Volume 29 Number 1 March 1978

BJPIA5 29 (No. 1) 1-116 (1978) ISSN 0007-0882

GUK-H00669-20-P1093

The British Journal for the Philosophy of Science

Published for the
British Society for the Philosophy of Science
by Aberdeen University Press

THE BRITISH JOURNAL FOR THE PHILOSOPHY OF SCIENCE

Editors: JOHN WATKINS and JOHN WORRALL
Assistant Editors: COLIN HOWSON and ELIE ZAHAR

Subscriptions from January 1978
The *Journal* is published quarterly. The subscription price for mailing addresses in Britain is £7.00 net (U.S.A. and Canada U.S. $22.00, other overseas £11.00) per volume post free; single issues £2.50 net (U.S.A. and Canada U.S. $8.00, other overseas £4.00) plus postage. For members of the British Society for the Philosophy of Science—who should order direct from the publisher—the subscription price is £4.50 (U.S.A. and Canada U.S. $14.00, other overseas £7.00). Membership of the Society is international and is by no means confined to people resident in the United Kingdom. Orders may be sent to any bookseller or direct to Aberdeen University Press, Farmers Hall, Aberdeen, Scotland AB9 2XT.

Notes to Contributors
Articles, correspondence and books for review should be sent to John Worrall at the Department of Philosophy, London School of Economics, Houghton Street, Aldwych, London WC2A 2AE. Articles should be accompanied by abstracts of about 100 words.

Material is accepted for publication in the *Journal* on the condition that copyright is assigned to the British Society for the Philosophy of Science.

Three copies of all contributions should be sent, typed on one side only with wide margins and double-spaced. Footnotes, which should be as few as possible, should be typed separately, double-spaced at the end of the text. They should be referred to by consecutive numbering throughout the text, in which they will be inserted in page proof. Bibliographical references should be listed alphabetically at the end. The style of references should follow current *Journal* practice.

Long quotations are printed in small type without quotes, and should be so marked in the typescript. Single quotes should be used to form names of words and for shorter quotations; double quotes for other purposes. Diagrams, tables, and illustrations should be on separate sheets, with their desired position in the text clearly indicated. Symbolic formulae appearing as separate lines should be clearly distinguished in the text, and foreign letters (Greek, German, etc.) indicated in the margin. Material to be printed in italics, especially English letters used as symbols ('*A*', '*x*', etc.), should be underlined. On matters of style and layout, authors should consult CHAUNDY, T. W. *et al.* [1964]: *The Printing of Mathematics*. London: Oxford University Press.

Offprints of articles may be ordered when corrected galley proofs are returned. Fifty are supplied free ; the price of extra copies is given on the order form.

© British Society for the Philosophy of Science 1978

For permission to reproduce material from *The British Journal for the Philosophy of Science*, please apply to the Editors.

Back Volumes
Volumes 1-16 may be obtained from Kraus Reprint Ltd., FL-9491 Nendeln, Liechtenstein or Kraus Reprint Corporation, 16 East 46 Street, New York, N.Y. 10017. Volumes 17-27 may be obtained from Aberdeen University Press, Farmers Hall, Aberdeen, Scotland AB9 2XT. Prices on application.

29

34

MEMBERS OF THE BRITISH SOCIETY FOR THE PHILOSOPHY OF SCIENCE*

[F = Foundation Member of the former Philosophy of Science Group
H = Honorary Member]

Abell (Dr. P. M.), Dept of Sociology, University of Essex, Colchester, Essex
Abraham (V. M.), 28 Ellington Road, London N10 3DG
Abrams (Prof. J. W.), 120 Glen Road, Toronto, Ontario M4W 2W2, Canada
Alexander (Prof. P.), Dept of Philosophy, The University, Bristol BS8 1RJ
Alves (L. F.), Rua Uruguai 468 apt Co 1, Tijuca ZC 09, Rio de Janeiro 20000, Brazil
Analytis (P.), Director, Greek Tourist Office, 195/7 Regent St., London WIR 8DL
Anderson (Prof. S.), School of Architecture & Planning, Room 4.209, Massachusetts Institute of Technology, Cambridge, Massachusetts 02139, U.S.A.
Andreski (Prof. S. L.), Dept of Sociology, University of Reading, Whiteknights Park, Reading
Andrews (A. J.), 14 Stansgate Ave., Cambridge CB2 2QZ
Angluin (D. J. C.), Dept of Sociology, The Polytechnic of North London, Ladbroke House, Highbury Grove, London N5 2AD
Anzola (G.), Flat 6, 19 Queensgate Gdns., London SW7
Armstrong (A. C.), School of Environmental Sciences, University of East Anglia, University Plain, Norwich NOR 88C
Austin (W. H.), 51 Rosebery Road, London N10 2LE
Ayer (Prof. Sir Alfred), F.B.A., New College, Oxford

Barker (Mrs. E. V.), Dept of Sociology, London School of Economics, Houghton Street, London WC2A 2AE
Barker (F.), 2 Knighton Lodge, 39 Rydens Road, Walton-on-Thames, Surrey KT12 3AG
Barlow (W.), 3 Albert Court, Kensington Gore, London SW7
Barrett (Dr. J. C.), Dept of Medical Statistics and Epidemiology, London School of Hygiene and Tropical Medicine, Keppel Street, London WC1
Bartley (Prof. W. W. III), Dept of Philosophy, California State University, Hayward, California 94542, U.S.A.
Bastin (Dr. E. W.), 11 Millington Road, Cambridge
Batens (Dr. D.), Philosophy Dept, Rijksuniversiteit Gent, Blandijnberg 2, B-9000 Gent, Belgium
Bear (D.), 99 Castlewood Drive, Eltham, London SE9

---

* *Corrected December* 1977

Belsey (A.), Dept of Philosophy, University College, PO Box 78, Cardiff CF1 1XD
Bennett (P. G.), 'Lichens', Hill Road, Lewes, Sussex
Beveridge (W. N.), 33 Bryson Road, (1st floor, 4), Edinburgh 11
Bhaskar (R. R.), Dept of Philosophy, University of Edinburgh, David Hume Tower, George Square, Edinburgh EH8 9JX
Biggs (R. C.), Dept of Linguistics, Sidgwick Ave., Cambridge CB3 9DA
Birner (J.), Beneden Oostzeedijk 213A, Rotterdam 3016, The Netherlands
Blachowicz (Prof. J. A.), Dept of Philosophy, Loyola University of Chicago, 6525 N. Sheridan Road, Chicago, Illinois 60626, U.S.A.
Blum, (Dr. A.), Dept of Philosophy, Bar-Ilan University, Ramat-Gan, Israel
Boden (Dr. Margaret A.), Dept of Philosophy, University of Sussex, Falmer, Brighton BN1 9QH
Boon (L.), 2E Jan Steenstraat 29^{II}, Amsterdam, The Netherlands
Botterill (G. B.), Dept of Philosophy, University of Central Wales, Aberystwyth, Dyfed
Bradley (Dr. J.), 38 New Walk, N. Ferriby, E. Yorkshire
F Braithwaite (Prof. R. B.), F.B.A., King's College, Cambridge
Brandon (E. P.), 175 Heath Road, Twickenham, Middlesex
Briskman (L. B.), Dept of Philosophy, University of Edinburgh, David Hume Tower, George Square, Edinburgh EH8 9JX
Bristol (T.), William Goodenough House, Flat 102, Mecklenburgh Square, London WC1
Brookes (W. M.), 9 Brook Gdns., Emsworth, Hants.
Brooks (A. C.), 521 Chaussée de Louvain, 1328, Ohain, Belgium
Brown (H. I.), Northern Illinois University, De Kalb, Ill. 60115, U.S.A.
Brown (J. M.), Dept of Philosophy, New University of Ulster, Coleraine, N. Ireland
Brown (J. R.), c/o Dept of Philosophy, University of W. Ontario, London, Canada
Buchdahl (G.), 11 Brookside, Cambridge
Bunge (Prof. M. A.), Dept of Philosophy, McGill University, Montreal, Canada
Burch (P. R. J.), Dept of Medical Physics, The General Infirmary, The University, Leeds 1
Burgos (A.), Universidad Nacional de Rio Cuarto, Depto de Matemática, Enlace Rotas 8 y 36 -Km 603, 5800 Rio Cuarto, Argentina
Burnett (M.), 52 Truro Road, Wood Green, London, N22
Buxton (J.), 13 De Verdun Ave., Belton, Loughborough, Leics. LE12 9TY

Cain (J. C.), 63 Park Road, Chiswick, London W4
F Caldin (Prof. E. F.), 3 London Road, Canterbury, Kent
Cassirer (Dr. E.), Berlin 33, Wildpfad 28, Germany
Chalmers (Dr. A. F.), Dept of Philosophy, University of Sydney, Sydney, N.S.W. 2006, Australia
Chao (Prof. Y. R.), Oriental Languages Dept, University of California, Berkeley, Calif. 94720, U.S.A.
Chapman (A. N.), 84 Henniker Road, Ipswich IP1 5HG
Chessick (Dr. R. D.), 2622 Park Place, Evanston, Ill. 60201, U.S.A.

P / v. 13

Clark (Dr. P. J.), Apt. 860, 6 Bayard Road, Pittsburgh, Pa 15213, U.S.A.
Clarke (Dr. D. M.), Philosophy Dept, University College, Cork, Ireland
Clarke (E. J. W.), 67 Walsingham Gdns., Stonleigh, Ewell, Sy
Clarke (J. D.), 5 Sutherland Grove, Bletchley, Bucks.
Cohen (L. J.), The Queen's College, Oxford
Cohen (Prof. R. S.), 44 Adams Avenue, Watertown, Mass., 02172, U.S.A.
Colclough (Dr. A. R.), Div. of Quantum Metrology, N.P.L., Teddington, Middlesex
Costall (A. P.), Dept of Psychology, The University, Southampton SO9 5NH
Cress (T. V.), 6 St Luke's Close, Woodside Court, South Norwood, London SE25
F Crombie (Dr. A. C.), Trinity College, Oxford
Crowther (A. B.), 176 Lytham Road, Wasten, Preston, Lancs.
Cunliffe (C. R. A.), 29 Riverdale Gardens, Twickenham, Middx.
Cunningham (A. D.), 24 rue du 9 mai, Howald, Luxembourg
Currie (G. P.), 2206 Haste, Berkeley, California 94704, U.S.A.

D'Agostino (F.), 62 Tubbs Road, London NW10 4RE
Davies (Dr. D.), 135 Aspen, Dollard des Ormeaux, Quebec, Canada H9A 2N9
Davies (P. S.), 38a Palmerston Cres., Palmers Green, London N13
Day (J. P.), Dept of Philosophy, The University, Keele, Staffs ST5 5BG
Dedek (J.), 19 Avenue Paul Langevin, F-92260 Fontenay-aux-Roses, France
de Grandy (The Count Lysander), Glen Eyre Hall, Bassett, Southampton SO9 2QN
Del Mar (R. H.), 8 Campden Hill Gate, Duchess of Bedford's Walk, London W8
Denbigh (Dr. K. G.), F.R.S., Principal, Queen Elizabeth College, Campden Hill Road, London W8
Denyer (Dr. G. E.), 11 School Lane, Harlow, Essex
de Vries (Dr. G. H.), Saffierstraat 12, Boekelo, The Netherlands
de Vries (R. P.), Nieuwe Kade 17, Utrecht, The Netherlands
Dickson (J.), 153 Kingsway, Hove 3, Sussex
Dieks (D.), Gouden Leeuw 1018, Amsterdam, The Netherlands
HF Dingle (Prof. H.), 118 Marlborough Ave., Hull HU5 3JX
Dolby (R. G. A.), Unit for the History, Philosophy and Social Relations of Science, Physics Laboratory, The University, Canterbury, Kent CT2 7NR
Dorling (J.), Dept of History and Philosophy of Science, Chelsea College, Manresa Road, London SW3 6LX
Drabble (C.), 11 Harcourt Road, Plaistow, Stratford, London E15
Duffy (M. C.), Mechanical Engineering Dept, Sunderland Polytechnic, Chester Road, Sunderland, Tyne and Wear
Duncan (A. M.), 116 Herrick Road, Loughborough, Leics.
Stack-Dunne (Dr. M.), Beatson Inst. for Cancer Research, 132 Hill St., Glasgow G3 6UD
Dyason (D.), Dept. of History and Philosophy of Science, University of Melbourne, Parkville 3052, Victoria, Australia

Ebison (M. G.), 84 College Road, Isleworth, Middlesex
Eccles (Sir John), F.R.S., Cà a la Grà Contra (LOCARNO) TI, CH 6611, Switzerland

Edge (Dr. D. O.), Science Studies Unit, University of Edinburgh, 34 Buccleuch Place, Edinburgh EH8 9JT

Edgington (D. M.), Dept of Philosophy, Birkbeck College, Malet St., London WC1

Edman (M.), Avdelningen för vetenskapsteori, 901 87 Umeå, Sweden

Ernest (P.), 15 Inglewood Road, London NW6 1QT

Evans (Dr. R. G.), Peterborough Technical College, Park Crescent, Peterborough PE1 4DZ

Everaars (Dr. J. L.), Logic and Scientific Method Division, Physics Building, Univ. of Sussex, Falmer, Brighton BN1 9QH

Evesham (H. A.), 8 Taylors Close, Meppershall, Shefford, Beds. SG17 5NH

Farrar (Dr. W. V.), Homeland, Navigation Road, Altrincham, Cheshire

Feltham (Prof. P.), Dept of Physics, Brunel University, Uxbridge, Middx. UB8 3PH

Fetherstonhaugh (P.), 14 Frognal, London NW3

Findlay (A. F.), 48 Homewood Avenue, Cuffley, Potters Bar, Middx.

Fingerman (J.), 5551 S. Ingleside, 3, Chicago, Ill. 60637 U.S.A.

Finocchiaro (Prof. M. A.), Dept of Philosophy, University of Nevada, 4505 Maryland Parkway, Las Vegas, Nevada 89154, U.S.A.

Fischer (Dr. R.), 1989 Greenberry Rd., Baltimore, Md. 21209, U.S.A.

Ford (Dr. O. E.), 12 Highgate Spinney, Crescent Road, London N8

Foreman (J. C.), Division of Clinical Immunology, Dept of Medicine, Johns Hopkins University School of Medicine, The Good Samaritan Hospital, 5601 Loch Raven Boulevard, Baltimore, Maryland 21239, U.S.A.

Forkosch (Dr. M. D.), Pace University School of Law, 78 North Broadway, White Plains, N.Y. 10603, U.S.A.

Freeland (Dr. G. A.), School of History and Philosophy of Science, University of New South Wales, P.O. Box 1, Kensington, N.S.W., Australia

Frické (M.), Dept of Philosophy, University of Otago, P.O. Box 56, Dunedin, New Zealand

Frielink, (A. B.), Albert Cuypstraat 216 4H, Amsterdam, The Netherlands

Gaines (Prof. B. R.), 13 Norman Way, Colchester, Essex

Gell (Prof. P. G. H.), F.R.S., Dept of Experimental Pathology, Medical School, Birmingham B15 2TJ

Geraghty (D. M.), 48 Poplicans Road, Cuxton, Nr. Rochester, Kent

Gibbins (P. F.), Dept of Philosophy, University of Hull, Hull HU6 77X

Giedymin (Dr. J.), 11 Harrington Villas, Brighton, Sussex BN1 6RG

Gillard (P. L.), Dept of Chemistry, Polytechnic of South Bank, Borough Road, London SE1

Gillen (Dr. H. W.), 1601 Doctor's Circle, Wilmington, N.C. 28401, U.S.A.

Gillies (D.), 'Clevedon', 128 Telford Ave., London SW2

Gillies (Dr. D. A.), Dept of History and Philosophy of Science, Chelsea College, Manresa Road, London SW3 6LX

Girill (Dr. T. R.), 3501C Colonial Drive, Visalia, California 93277, U.S.A.

Goldman (Dr. A.), Dept of Philosophy, University of Michigan, Ann Arbor, Mi 48109, U.S.A.

Gonley (J.), 12 Gardenia Avenue, Luton, Bedfordshire

Good (Prof. I. J.), Dept of Statistics, College of Arts and Sciences, Virginia Polytechnic Institute, Blacksburg, Virginia 24061, U.S.A.
Gooddy (Dr. W.), University College Hospital, Gower Street, London WC1
Gooding (D. C.), School of Humanities and Social Sciences, University of Bath, Claverton Down, Bath BA2 7AY
Gower (B. S.), 50 Old Elvet, Durham DH1 3HN
Grant (Dr. E.), P.O. Box 1356, Sydney, Nova Scotia, Canada B1P 6K3
Grant (M.), 110 Dukes Road, Southampton, Hants.
Grant (N.), 67 Drumgelloch St., Airdrie, Lanarkshire
Green (M. R.), 1804 N. Rosebrook Lane, Rosemead, Cal. 91770, U.S.A.
Green (Rev. T. H.), San José Seminary, P.O. Box 4475, Manila, Philippines
Greenaway (Dr. F.), Science Museum, London, SW7 2DD
Grieder (A.), Dept of Social Science and Humanities, City University, St. John Street, London EC1 4PB
Anthanasiou-Grivas (Dr. D.), Dept of Civil Engineering, Rensselaer, Polytechnic Inst., Troy, N.Y. 12181, U.S.A.
Grünbaum (Prof. A.), Center for Philosophy of Science, 2509 Cathedral of Learning, University of Pittsburgh, Pennsylvania 15260, U.S.A.
Gruner (Dr. H. J. R.), Dept of Philosophy, University of Sheffield, Sheffield S10 2TN

Haack (Dr. R. J.), Dept of Philosophy, University of Warwick, Coventry CV4 7AL
Halsbury (The Rt. Hon. The Earl of), Flat 4, Campden House, 29 Sheffield Terrace, London W8 7NE
Hann (G. H.), 31 Poulner Close, Bognor Regis, Sussex PO22 8HN
Harnad (S. R.), c/o Paul Oppenheim Esq., 57 Princeton Ave., Princeton, N.J. 08540, U.S.A.
Harré (R.), Linacre College, Oxford
Harrison (Prof. J.), 42 Broadgate, Beeston, Nottingham
Hart (P.), Dept of Zoology, School of Biological Sciences, Adrian Building, University Road, Leicester LE1 7RH
Hartman (F. A.), Dept of Botany, University of Michigan, Ann Arbor, Michigan 48104, U.S.A.
Heelan (Prof. P. A.), S.J., Dept of Philosophy, State University of New York, Stonybrook, Long Island, New York 11790, U.S.A.
Hegenberg (Dr. L. H. B.), I.T.A., São José dos Campos, São Paulo 12.200, Brazil
Hempel (Prof. C. G.), Dept of Philosophy, 1879 Hall, Princeton University, Princeton, New Jersey 08540, U.S.A.
Henning (J. L.), Philosophy Dept, University of Colorado, Boulder, Co. 80309, U.S.A.
Hesse (Prof. Mary B.), F.B.A., Wolfson College, Cambridge
Higgs (Dr. P. W.), Dept of Physics, University of Edinburgh, James Clerk Maxwell Bldg, Mayfield Road, Edinburgh EH9 3JZ
Hirschmann (H. D.), Dept of Philosophy, The University, Bristol BS8 1RJ
Hoare (Dr. M. R.), 20 Bradbourne Street, London SW6

Hodgkin (A. G.), Oxford University Press, Academic Division, Walton St., Oxford OX2 6DP
Howson (C.), Dept of Philosophy, London School of Economics, Houghton Street, London WC2A 2AE
Hughes (M. W.), Dept of Philosophy, University of Newcastle-upon-Tyne NE1 7RU
Hughesdon (Dr. P. E.), 107 Corringham Road, London NW11
Hung (Dr. H.-C.), Philosophy Dept, Waikato University, Hamilton, New Zealand
Hunt (G. M. K.), Dept of Philosophy, University of Warwick, Coventry CV4 7AL

Ironmonger (Dr. E. E.), Westminster College, North Hinksey, Oxford
Isaacson (Dr. D.), St John's College, Oxford OX1 3JP
Ishiguro (H.), Dept of Philosophy, University College, London WC1

Jackson (R. B.), The Cottage, 9C The Grove, London N6 6JU
Jarvie (Prof. I. C.), Dept of Philosophy, York University, Toronto 12, Canada
Jason (G.), 21165 Escondido St., Woodland Hills, Cal. 91364, U.S.A.
Jenner (Prof. F. A.), Middlewood Hospital, Sheffield S6 1TT
Johnson (L.), Dept. of Cybernetics, Brunel University, Uxbridge, Middx.
Jones (K. E.), Faculty of Humanities, Darwin College, The University, Canterbury, Kent
Jordan (D. P.), School of Humanities, Dept of Philosophy, La Trobe University, Bundoora, Victoria, Australia 3083
Justesen (Dr. D. R.), Veterans Administration Hospital, 4801 Linwood Boulevard, Kansas City, Missouri 64128, U.S.A.

Kadifachi (S.), Physics Dept, Birkbeck College, Malet St, London WC1
Kaiser (C. B.), Western Theological Seminary, Holland, Michigan 49423, U.S.A.
Kamino (K.), 25–122 Momoyama-Cho-Oshima, Fushimi-Ku, Kyoto, Japan
Kannegiesser (H.), Faculty of Arts, Swinburne College of Technology Ltd., P.O. Box 218, Hawthorn, Melbourne, Australia 3122
Kasher (Dr. A.), Dept of Philosophy, Bar-Ilan University, Ramat-Gan, Israel
Keaney (Dr. B. P. P.), Dept of Philosophy, University of Cape Town, Rondebosch, C.P., South Africa
Kellett (Dr. B. H.), Dept of Physics, University of Melbourne, Parkville, Victoria 3052, Australia
Kenny (V.), Garden Hill House, Professorial Unit, Dept of Psychiatry, University College, 1 James's St., Dublin 8
Kilmister (Prof. C. W.), King's College, Strand, London WC2
Kitch (J. M.), 94 Malden Road, Kentish Town, London NW5
Klein (H. R.), 8 Uxendon Hill, Wembley, Middlesex HA9 9RU
Kneale (Prof. W. C.), F.B.A., 4 Bridge End, Grassington, Nr. Skipton, N. Yorkshire BD23 5NH
Knox (W. C.), High Orchard, Mark Way, Godalming, Surrey
Koertge (Prof. N.), Dept of History and Philosophy of Science, Goodbody Hall 130, Indiana University, Bloomington, Indiana 47405, U.S.A.

Kollas, (Dr. J. G.), 6 Polyzoidou St., Athens 702, Greece
Körner (Prof. S.), F.B.A., Dept of Philosophy, The University, Bristol BS8 1RJ

Lal (S.), Dept of Physiology and Pharmacology, Chelsea College, Manresa Road, London SW3 6LX
Lambert (E. W.), 65 Alexandra Road South, Manchester M16 8GJ
Landau (R. S.), 10 Ladbroke Walk, London W11
F Landsberg (Prof. P. T.), 118 Highfield Lane, Southampton
Largeault (Dr. J.), 2 rue de Feuillants, 31300, Toulouse, France
Latsis (Dr. S. J.), 7 Deanery St., Park Lane, London W1Y 5LH
Lavis (D. A.), Dept of Mathematics, Chelsea College, Manresa Road, London SW3 6LX
Leach (Prof. Sir Edmund), 52 Storey's Way, Cambridge
Ledermann (Dr. E. K.), 8 Elsworthy Road, London NW3
Lejewski (Prof. C.), Dept of Philosophy, University of Manchester, Manchester 13
Leplin (Dr. J.), Dept of Philosophy, University of N. Carolina, Greensboro, N.C. 27410, U.S.A.
Levi (Prof. I.), Philosophy Dept, Philosophy Hall, Columbia University, N.Y., N.Y. 10027, U.S.A.
Lindenmayer (Prof. A.), Theoretical Biology Group, Rijksuniversiteit Utrecht, Padualaan 8, Utrecht 2506, The Netherlands
Lowe (M. F.), Van Leer Institute, 43 Jabotinsky Street, Jerusalem, Israel
Lyon (Dr. A. J.), Dept of Social Science and Humanities, City University, St John Street, London EC1 4PB

McEvoy (J. G.), Dept of Philosophy, University of Cincinnati, Cincinnati, Ohio 45221, U.S.A.
McEwen (G. T.), 6 Heatherlea Drive, Kintillo, Bridge of Earn, Perth
McGowan (R. S.), Dept of Philosophy, The University, Leicester
McMorris (Dr. M. N.), Dept of Physics, University of the West Indies, Mona, Kingston 7, Jamaica, W.I.
Machover (Dr. M.), Chelsea College, Manresa Road, London SW3 6LX
Mackay (C. J.), Dept of Psychology, University of Nottingham, Nottingham NG7 2KD
Mackay (Prof. D. M.), University of Keele, Staffs.
Mackie (J. L.), University College, Oxford
Magee (B.), 12 Falkland House, Marloes Road, London W8
Malin (H.), 34 Essex Park, London N3 1NE
Manser (Prof. A. R.), Dept of Philosophy, The University, Southampton
Margolin (M.), 4 Surrey Lane, Livingston, N.J. 07039, U.S.A.
Markham (K. A.), Dept of Applied Psychology, U.W.I.S.T., Cardiff
Marks (D. H.), Flat 5, 11 Kings Gdns., Hove, Sussex
Martins (H. G.), St Anthony's College, Oxford OX2 6JF
F Mason (Prof. S. F.), Dept of Chemistry, Kings College, London WC2
Maxwell (A. N.), Dept of the History and Philosophy of Science, University College, London WC1
Mayne (A. J.), 63a Muswell Avenue, London N10

F Mayo (Prof. B.), University of St. Andrews, Fife, Scotland
Mays (Dr. W.), Dept of Philosophy, The University, Manchester 13
Mellor (Dr. D. H.), Darwin College, Cambridge
Merrett (T. H.), School of Computer Science, McGill University, Montreal, P.Q., Canada
Merrill (D. D.), Dept of Philosophy, Oberlin College, Oberlin, Ohio 44074, U.S.A.
Miller (D. W.), Dept of Philosophy, University of Warwick, Coventry CV4 7AL
Millman (A.), Dept of Philosophy, University of British Columbia, Vancouver BC V5T IW5 Canada
Molland (A. G.), Dept of History and Philosophy of Science, King's College, Aberdeen
Montgomery (Prof. H. A.), 83 Seaview Road, Remuera, Auckland 5, New Zealand
Musgrave (Prof. A. E.), Dept of Philosophy, University of Otago, P.O. Box 56, Dunedin, New Zealand

Nakasai (T.), 2-2-18 Akai, Daito-Shi, Osaka-Fu, 574, Japan
Nidditch (Prof. P. H.), Dept of Philosophy, The University, Sheffield S10 2TN
Barndorff-Nielsen, (Prof. O.), Afdeling for Teoretisk Statistik, Matematisk Institut, Aarhus Universitet, 8000 Aarhus C, Denmark
Niiniluoto (Prof. I. M. O.), Lammaslammentie 13 F 103, 01710 Vantaa 71, Finland
Nola (Dr. R.), Dept of Philosophy, University of Auckland, Private Bag, Auckland, New Zealand
North (Dr. J. D.), 28 Chalfont Road, Oxford

O'Connor (Prof. D. J.), Dept of Philosophy, The University, Exeter
O'Hear (Dr. A.), Dept of Philosophy, University of Surrey, Guildford, Surrey
Olsen (Prof. D. E.), 771 San Diego Road, Berkeley, California 94707, U.S.A.
Owen (C. F.), Centre for the Study of Human Development, University of London Institute of Education, 58 Gordon Square, London WC1H 0NT
F Owen (Dr. E. C.), The Hannah Dairy Research Institute, Kirkhill, Ayr

Pähler (K.), Essener Str. 30, 46 Dortmund, W. Germany
Parkinson (K. A.), 30 St James Street, Southport, Lancs.
Parry (Mrs. K. Croose), 81 Onslow Square, London SW7
Pask (A. G. S.), No address known
Patterson (N. D.), 15 John Cobb Road, Weybridge, Surrey
Peacock (Dr. T. E.), Dept of Chemistry, University of Queensland, St Lucia, Brisbane, Queensland 4067, Australia
Pearce (D. A.), 17 Brunswick Place, Hove, Sussex
Pleydell-Pearce (A. G.), Dept of Extramural Studies, University of Birmingham, P.O. Box 363, Edgbaston, Birmingham 15
Pleydell-Pearce (I. W.), 13 Southfield Road, Bristol 6
Phillips (C.), 56 Shaftesbury Road, Brighton, Sussex
F Pirenne (Dr. M. H.), University Laboratory of Physiology, Oxford

Pirie (Dr. N. W.), F.R.S., Rothamsted Experimental Station, Harpenden, Herts.
Birr-Pixton (Hon J. W. W.), 3 Badger, Shropshire, WV6 7JP
Plaut (H. C.), 40 Tracery Park Road, Banstead, Surrey
F Popper (Prof. Sir Karl), F.B.A., F.R.S., Fallowfield, Manor Close, Manor Road, Penn, Bucks.
Post (Prof. H. R.), Chelsea College, Manresa Road, London SW3 6LX
Powers (J. H.), Enfield College of Technology, Queensway, Enfield, Middlesex
Prokhovnik (Prof. S. J.), School of Mathematics, University of New South Wales, Kensington, N.S.W., 2033, Australia
Pursey (M.), 51 Clyst Valley Road, Clyst St. Mary, Exeter, Devon
Purton (A. C.), 8 Norwich Road, Chedgrave, Norwich

Radnitzky (Prof. G.), Im Obersten Garten 48, D-5501, Gutweiler, W. Germany
Ravest (J. F.), 44 Oakland Drive, Upton, Wirral, Merseyside
Ravetz (Dr. J. R.), 5 Moor Park Ave, Leeds 6
Redhead (Dr. M. L. G.), 34 Coniger Road, London SW6
Richards (Prof. J.), Dept of Philosophy, University of Georgia, Athens, Georgia 30602, U.S.A.
Riley (R. I.), 6 South Rise, St George's Fields, Bayswater, London W2
Robson (T. G.), Durrwood Hall, Endon, Stoke-on-Trent, Staffs.
Rogers (G. A. J.), Dept of Philosophy, University of Keele, Staffs ST5 5BG
Roos (W. J.), Willem Barentszstraat 58, Utrecht, The Netherlands
Roper (M. J.), 21 Pentland Gardens, Edinburgh EH10 6NN
Rose (R. H.), 36 Willow Road, Finchfield, Wolverhampton, Staffs.
Roseveare (N. T.), 1 Cranford View, Salterton Rd, Exmouth, Devon EX8 2EH
Ross (G.), Dept of Physics, Queen Elizabeth College, University of London, Campden Hill Road, London W8 7AH
Roxburgh (Prof. I. W.), Dept of Applied Mathematics, Queen Mary College, Mile End Road, London E1 4NS
Rozeboom (Prof. W. W.), Dept of Psychology, University of Alberta, Edmonton, Alberta, Canada
Ruskin (P.), Flat 22, 93 Elm Park Gardens, Chelsea, London SW10
F Russell (The Revd. J. L.), Heythrop College, 11 Cavendish Square, London W1M 0AN

Sachs (Prof. M.), Dept of Physics, State University of New York, Buffalo, N.Y. 14214, U.S.A.
Salaman (M. H.), 23 Bisham Gdns., Highgate, London N6 6DJ
Salmon (Prof. W. C.), Dept of Philosophy, University of Arizona, Tucson, Arizona 85721, U.S.A.
Sampson (G. R.), Dept of Linguistics, University of Lancaster LA1 4YT
Samuels (W. E. S.), 38 Montpelier Road, Purley, Surrey CR2 2QA
Saraga (Dr. W.), 43 Kingsway, Petts Wood, Orpington, Kent
Savage (Dr. S. W.), Windover, Ingon Lane, Stratford-on-Avon, Warwicks.
Schaffner (Prof. K. F.), Dept of History and Philosophy of Science, University of Pittsburgh, Pittsburgh, Pa. 15260, U.S.A.
Scoledes, (Prof. A. M.), 2770 Frontier Trail, Chamblee, Georgia 30341, U.S.A.

Scott, (Dr. G. P.), Dept of Chemistry, University of S. Dakota, Vermillion, S.D., U.S.A.
Sendak (J. R.), 73 Brookland Rise, London NW11 6DT
Serry (V.), Flat 1, No. 6, The Paragon, Blackheath, London SE3
Settle (Prof. T. W.), Philosophy Department, University of Guelph, Guelph, Ontario, Canada
Sewell (E. J.), North East Flat, Bede College, Durham
Shearer (M.), City of E. London College, Pitfield St., London N1
Shearmur (J. F. G.), 96 Finsbury Park Road, London N4
Sheiham (H.), Dept of Philosophy, London School of Economics, Houghton St., London WC2A 2AE
Shigeo (I.), Suzaka P.O. Box 8, Shin-machi, Suzaka-Shi, Nagano-Ken, Japan
Shott (P. R.), 55 Frederick Ave., Penkhull, Stoke-on-Trent ST4 7DY
Siegel (H.), Dept of History and Philosophy of Education, 111 Teachers College, University of Nebraska–Lincoln, Lincoln, Nebraska 68588, U.S.A.
F Silver (Prof. R. S.), Dept of Mechanical Engineering, The University, Glasgow W2
Sim (Group-Captain J. W.), Pine Cottage, Harfield Road, Sunbury on Thames, Middx.
Simopoulos (D. P.), Eugenides Planetarium, Syngrou Ave., Amphithea, Athens, Greece
Singer (Dr. K.) 54 Simons Walk, Egham, Surrey
Sloman (Dr. A.), Arts Building, University of Sussex, Falmer, Brighton BN1 9QN
Smith (J. P.), 30 Orlin Avenue S.E., Minneapolis, Minn. 55414, U.S.A.
Newton-Smith (W.), Balliol College, Oxford OX1 3BJ
Sofroniou (Dr. S. A.), 21 Ferncroft Avenue, London NW3
Spector (Prof. M.), 7 Brandywine Drive, Setauket, New York 11733, U.S.A.
Stonier (Prof. A. W.), c/o Rex Hotel, Queen St., Auckland, New Zealand
Stopes-Roe (Dr. H. V.), 155 Moor Green Lane, Moseley, Birmingham 15
Strong (J. P.), Ministry of Defence Future Systems Studies MVEE, Chobham Lane, Chertsey, Surrey
Stuewer (Prof. R. H.), School of Physics and Astronomy, University of Minnesota, Minneapolis 55455, U.S.A.
Swinburne (Prof. R. G.), Dept of Philosophy, University of Keele, Keele, Staffs ST5 5BG
Szabo (G. S. A.), 41 King's Way, Harrow, Middlesex HA1 1XT

Tarkowski (Z. M.), School of Mathematical and Physical Sciences, Maths and Physics Building, University of Sussex, Falmer, Brighton BN1 9QH
Thompson (R. P.), Division of Humanities (Philosophy), Scarborough College, University of Toronto, West Hill, M1C 1A4, Ontario, Canada
Thonemann (P. H.), (Address unknown)
F Thorpe (Dr. W. H.), F.R.S., Jesus College, Cambridge
Torrance (Dr. T. S.), Dept of Logic, University of Aberdeen, King's College, Aberdeen AB9 2UB
Toulmin (Prof. S. E.), Committee on Social Thought, University of Chicago, Chicago, Ill. 60637, U.S.A.

Trenn (Prof. T. J.), Dept of History of Science, Universität Regensburg, 8400 Regensburg, Universitätsstrasse 31, Postfach, Germany.
Tuomela (Prof. R. H.), Department of Philosophy, University of Helsinki, Unioninkatu 40B, 00170 Helsinki, Finland

Urbach (Dr. P. M.), Philosophy Dept, London School of Economics, Houghton St, London, WC2A 2AE

van der Vet (P. E.), W. Barentszstraat 19, Utrecht, The Netherlands
van Valen (Dr. L.), Biology Dept, University of Chicago, 1103 East 57th St, Chicago, Illinois 60637, U.S.A.

Ward (E. P.), Trio, Carlton Road, South Godstone, Surrey
Warner (L. S.), 62 Beech Road, St Albans, Herts AL3 5AT
Watkins (Prof. J. W. N.), London School of Economics, Houghton Street, Aldwych, London WC2A 2AE
Watson (Dr. J. H. P.), Westbourne House, 74 Truro Road, St. Austell, Cornwall PL25 5JS
Weber (B. C.), 25 Andrews Road, Earley, Reading, Berks RG6 2PJ
Webster (D.), 21 Waterpark Road, Broughton Park, Salford, Lancs M7 0FT
Westphal (J.), Shepherd's Corner, Tompset's Bank, Forest Row, Sussex
Wetherick (Dr. N. E.), Dept of Psychology, The University, Aberdeen
Whitrow (Mrs. A. M.), 41 Home Park Road, Wimbledon, London S.W.19
F Whitrow (Prof. G. J.), F.R.A.S., Dept of Mathematics, Imperial College, London SW7
F Wightman (Dr. W. P. D.), F.R.S.E., 112 Rutten Lane, Yarnton, Oxon
Wilks (Dr. Y.), Dept of Linguistics, University of Essex, Wivenhoe Park, Colchester, Essex
Williams (J. E. M.), 46 Miskin Road, Dartford, Kent
Williams (Dr. P. M.), Physics Building, University of Sussex, Falmer, Brighton BN1 9QH
Wilson (R. D.), 86 Bramhall Lane South, Bramhall, Stockport SK7 2EA
Winokur (Prof. S.), Dept of Psychology, Texas Christian University, Fort Worth, Texas 76129, U.S.A.
Winter (A. J. S.), Mount Pleasant Hotel, Calthorpe St., London WC1X 0HL
Winter (Dr. H. J. J.), 39 Tollards Road, Countess Weir, Exeter
Wisdom (Prof. J. O.), Dept of Philosophy, York University, Toronto, Ontario, Canada
Wood (Dr. R. H.), 290 Hempstead Road, Watford, Herts.
Worrall (Dr. J.), Philosophy Dept, London School of Economics, Houghton Street, Aldwych, London WC2A 2AE
Wright (C. B.), Dept of Philosophy, University of Exeter, Exeter, Devon

Young (D. A.), School of Social Sciences, University of Sussex, Falmer, Brighton BN1 9QH

Zahar (Dr. E. G.), Dept. of Philosophy, London School of Economics, Houghton Street, Aldwych, London WC2A 2AE
Zenkteler (M.), Psychology Dept, Banstead Hospital, Sutton, Surrey
Zimmerman (Prof. E. J.), Behlen Laboratory of Physics, University of Nebraska-Lincoln, Lincoln, Nebraska 68588, U.S.A.

Brit. J. Phil. Soc. 29 (1978), 1–23 *Printed in Great Britain*

501.05
B774

On the Neyman–Pearson Theory of Testing*

by SPENCER GRAVES

1 *Introduction.*
2 *What is Neyman–Pearson Theory?*
3 *The Equivalence between Bayesian and Neyman–Pearson Tests.*
4 *Likelihoods.*
5 *Summary.*

1 INTRODUCTION

Spielman [1973] presents 'A Refutation of the Neyman–Pearson Theory of Testing'. He states at the outset [p. 202],[1] 'I intend to show that NPT is inadequate on its own terms'. We have had great difficulty in following his argument because he never quite states exactly what Neyman–Pearson Theory (NPT) is to him, and because his objections are not relevant to our conception of NPT.

As we understand Spielman's criticism, it amounts to saying that NPT is inadequate because it is neither Bayesian nor reasoning based on the likelihood principle. We find this roughly equivalent to saying he is going to criticise Hebrew theology on its own terms, and then basing his entire argument on the New Testament. We hope to establish below that (1) Neyman and Pearson considered a Bayesian approach and reasoning based directly on likelihoods, and deliberately excluded both from their theory giving reasons for this exclusion and (2) the procedure Spielman would employ in any of the examples he discussed or cited—given the constraints of the problem—is exactly the one that Neyman and Pearson would recommend. There are differences between Neyman–Pearson and Bayesian Theory, but these differences are not relevant to the examples and the issues Spielman discusses.

---

* I would like to thank my friend and teacher Roger Carlson for encouraging me in this project and for criticising numerous versions of this paper. I would also like to thank Professors Jerzy Neyman and Stephen Spielman for reading and commenting on an earlier draft, and especially Professor Egon S. Pearson whose detailed comments clarified certain issues.

[1] This paper is largely a rebuttal of Spielman [1973]. For brevity, the numerous references to it will cite page number only.

Received 1 *September* 1976

Most of the statisticians of the late nineteenth and early twentieth centuries were Bayesians. The 1920s and 1930s witnessed a swing to frequentist statistical philosophy under the leadership of such figures as Jerzy Neyman, E. S. Pearson and R. A. Fisher. The pendulum is now returning to Bayesian philosophy following the inspired work of Abraham Wald (in statistical decision functions), L. J. Savage, and many, many others. However, a work based on the 'modern' Bayesian and likelihood philosophies can hardly constitute 'A Refutation of the Neyman–Pearson Theory of Testing', as it most certainly is not 'on its own terms'.

We begin in section 2 by defining NPT as we perceive it, in order to minimise further confusion. We then proceed in subsequent sections to compare and contrast this with Spielman's main objections, assuming for purposes of discussion various modifications to our conception of NPT as outlined in section 2. Almost all of Spielman's presentation carries a 'frequentist Bayesian flavour' [p. 206]; we discuss this in section 3. Spielman seems to assume throughout his discussion that the option of suspending judgment is always available; we discuss this along with likelihoods in section 4.

We are not saying that NPT is better than the approaches Spielman recommends, but only that Neyman and Pearson rejected these approaches stating unequivocally why they did so, and Spielman failed to acknowledge their arguments.

2 WHAT IS NEYMAN–PEARSON THEORY?

Spielman's source for a definition of Neyman–Pearson Theory (NPT) seems to be Neyman [1950, 1957]. He cites Pearson in Pearson and Kendall [1970] and Barnard and Cox [1963], but concludes [p. 202]: 'One gets the impression . . . that Pearson did not concern himself with the philosophical aspects of NPT after the publication of [Neyman and Pearson's early joint] papers'. We find Pearson's more recent comments relevant[1] and in spite of Spielman's remark [p. 202] that 'the early, joint papers of Neyman and Pearson are mostly concerned with the mathematical aspects of the theory', we find these papers most illuminating towards understanding the foundations of their theory. In particular, we expect to show through quotes from these and more recent works that Neyman and Pearson specifically excluded Bayesianism and reasoning based directly on likelihoods from the theory which bears their names.[1]

---

[1] This must be interpreted in light of Pearson's remark in Barnard and Cox [1962], p. 53:

> There were a good many references in the previous contributions to the Neyman and Pearson theory; they did not altogether correspond to the theory as I see it, but that perhaps is because my own views have changed somewhat and developed.

Pearson even questions the terminology as in [1967], p. 74:

> . . . I use the term Neyman and Pearson *approach* rather than Neyman and Pearson *theory*, because this seems to me the more appropriate term.

Neyman–Pearson theory may be characterised by the following: (*a*) One must choose between a hypothesis H and an alternative A, without, for example, the option of suspending judgment. (*b*) Bayes theorem was considered, and reasoning based on it was specifically *excluded* from NPT—not because it is wrong (it is after all a theorem in mathematics), but rather because the information required to use it was felt to be rarely available. (*c*) The 'pre-trial point of view' was adopted, and the 'post-trial point of view' excluded from NPT. (*d*) The size and power of a test were chosen as the primary concepts with which different tests could be compared. Using these concepts, they proved the now-famous Neyman–Pearson lemma which states that in testing a simple hypothesis H against a simple alternative hypothesis A, the test based on the likelihood ratio maximises the power for fixed test size. In the remainder of this section, Spielman's presentation of Neyman–Pearson Theory is summarised and contrasted with quotes from Neyman and Pearson.

(*a*) *Choosing H or A*

In discussing 'The Conceptual Framework of NPT', Spielman quotes Neyman [1957] on 'Inductive Behavior as a Basic Concept of Philosophy of Science'. He summarises his interpretation of this as follows [p. 203]:

> . . . the theory of testing should be viewed as a theory of prudential decision-making. According to NPT, problems of testing involve two contemplated courses of action, one which is the best to take if some statistical hypothesis H is true, and another which is the best to take if some hypothesis A is true.[2]

---

[1] Pearson [1962, p. 276] comments:

> . . . I believe rather strongly in the value of emphasizing continuity as well as differences in statistical philosophy. I am convinced that if we can only get to the bottom of the way in which similar situations are tackled by different approaches, all I believe lying within the broad path of development of our subject, our understanding will gain in richness—gain in a way which can never happen if we waste energy in trying to establish that we are right and the other fellow is wrong.

We hope that this paper will contribute to an understanding of the interrelationships between differing statistical philosophies, and that Professor Pearson will not judge us too harshly for this present effort.

[2] In a personal communication, Spielman [1976] advised the author, 'As I view it, N & P implicitly derive their formal theory of testing . . . under the postulate that prior probabilities are not known, or in some cases, not even definable. The crucial step is their conclusion that informative indices of the reliability of decisions to accept or reject hypotheses are obtainable only if the option of suspending judgement is omitted.' Our reaction is spelled out in this section.

Regarding notation, we note that in this section Spielman denoted the alternative hypothesis by H'; in other places, he used H_1 or H_1^T. Neyman and Pearson used various notations throughout their work. For internal consistency, we will use H for the hypothesis and A for the alternative, and translate quotes to this notation wherever it seems not to affect the meaning.

This is relatively accurate. In [1933*a*], p. 142,[1] Neyman and Pearson state:

> Without hoping to know whether each separate hypothesis is true or false, we may search for rules to govern our behavior with regard to them, in following which we insure that, in the long run of experience, we shall not be too often wrong. Here, for example, would be such a 'rule of behaviour': to decide whether a hypothesis, H, of a given type be rejected or not, calculate a specified character, x, of the observed facts; if $x>x_0$ reject H, if $x<x_0$ accept H. Such a rule tells us nothing as to whether in a particular case H is true when $x<x_0$ or false when $x>x_0$. But it may often be proved that if we behave according to such a rule, then in the long run we shall reject H when it is true not more, say, than once in a hundred times, and in addition we may have evidence that we shall reject H sufficiently often when it is false.

However, Spielman seems to think that 'inductive behaviour' or 'prudential decision-making' must include the option of suspending judgment if the data are insufficient. In discussing possible objections to his 'refutation', he notes [p. 214]:

> Another argument is that the problem calls for a decision for every outcome. . . . This may be a tolerable defense in some industrial problems, but in general it is quite weak.

Neyman and Pearson did mention the possibility of suspending judgment (*e.g.* [1928*a*, p. 1]). However, it is clear from other comments they made and all their mathematics that the possibility of suspending judgment was never intended to be a formal part of the theory. Rather, it was left as an informal part of the analysis. The behaviour assigned to the decision 'accept H' might, for example, be to indicate that the data were not sufficient. For example, in [1933*a*], p. 146 they remark:

> We may accept or we may reject a hypothesis with varying degrees of confidence; or we may decide to remain in doubt. But whatever conclusion is reached the following position must be recognized. If we reject H, we may reject it when it is true; if we accept H, we may be accepting it when it is false, that is to say, when really some alternative A_t is true.

Neyman and Pearson were clearly considering situations in which the number of observations was fixed as a part of the problem. The option of suspending judgment was not available. In [1947], Wald generalised their results to cover this case, as we point out in section 4 *below.*

---

[1] All references to Neyman and Pearson give the original date. However, the page numbers refer to the page in the reprinted collections rather than the original papers. We apologise for this rather unusual style of citation, but we feel that the original date of the references contributes somewhat to the discussion, while the collected papers tend to be more widely available.

(*b*) *NPT Excludes Bayes*

Neyman and Pearson are two of the principle proponents of the frequentist school of statistics. They state repeatedly that prior probabilities should be used whenever the prior is provided with the problem treated, but they avoid them otherwise and clearly omit their consideration from NPT.[1] Indicative of this attitude is the following statement from their first joint paper [1928*a*], p. 2 (see also [1928*a*], p. 12):

> . . . there is a common tendency to view with suspicion any method involving the use of inverse probability.

In contrast to this, Spielman [p. 205] notes that Neyman and Pearson talk in terms of the 'long run success' interpretation of probabilities:

> . . . the application of a test in a particular case is to be justified by considering what its performance would be for a long run of similar cases.

He then asserts [p. 205–6]:

> It would appear, then, that the conceptual framework of NPT as developed by its originators must incorporate a presupposition something like the following: For every test T there is a large open class R of test problems of the 'same type' . . . this presupposition lends itself to the view that it . . . is meaningful to talk about the (usually unknown) prior probability that an H-like hypothesis associated with R is true. . . . Thus the conceptual framework of NPT is quite congenial to a frequentist Bayesian approach to testing. . . . In fact, in his [1957] Neyman makes it quite clear that he has no objection to the use of Bayes' formula in connection with prior probabilities of hypotheses, provided that the approach is of the 'inductive behavior kind', and the probabilities are not conjured up in an *a priori* manner.

Spielman is reasonably correct in this regard, except for the implication that Bayesian methods might be included in NPT. We shall let Neyman speak for himself from the reference Spielman cited [1957], p. 19:

> Perhaps because of the lack of clarity in some of my papers, certain authors appear to be under the impression that, for some reason, I condemn the use of Bayes' formula and that I am opposed to any consideration of probabilities *a priori*. This is a misunderstanding. What I am opposed to is the *dogmatism* which is occasionally apparent in the application of Bayes formula when the probabilities *a priori* are not implied by the problem treated and an author attempts to impose on the consumer of statistical methods the particular *a priori* probabilities invented by himself for this particular purpose.

---

[1] Neyman and Pearson looked for tests which were *independent* of prior probabilities. In Barnard and Cox [1962], p. 55, Pearson remarks that in Neyman and Pearson [1933*b*], 'We discussed the testing of statistical hypotheses *a priori*; just because the problem of specifying the numerical values of these probabilities seemed to us so often insoluble, our aim here was to discuss in what sense the conclusions drawn from a test could be described as independent of these probabilities'.

At the risk of belabouring the point, we note that Pearson [1962], p. 277 (also in Barnard and Cox [1962], p. 55) commented on the philosophy behind his earlier work with Neyman.

> We were certainly aware that inferences must make use of prior information and that decisions must take account of utilities, but after some considerable thought and discussion round these matters we came to the conclusion, rightly or wrongly, that it was so rarely possible to give sure numerical values to these entities, that our line of approach must proceed otherwise.[1]

In [1962], Pearson applies Bayesian methods to a hypothesis testing problem. His point is our point: The Bayesian and Neyman–Pearson philosophies are different but in many cases dictate the same procedures, and he feels more comfortable with the frequentist justification.

Regarding Spielman's discussion of a reference class *R* for the test, we find it easier to conceive of separate reference classes for the hypothesis *H* and its alternative *A* than we do of a joint reference class *R* for both. Even if we accept the joint reference class *R*, it should be clear from the above that Neyman and Pearson would recommend we ignore it in the 'usual' case in which the proportion p of *H*-like hypotheses is unknown.[2] (Spielman's remark quoted above that p is usually unknown must be interpreted in light of his later statement [p. 213] that 'there usually exists a reference class the decision-maker is quite willing to adopt and for which he has enough information to set reasonable, informative upper and/or lower bounds to the truth-frequencies of the relevant hypotheses'.)

(*c*) *The Post-Trial Point of View*

Spielman [p. 201] refers to Hacking [1965] when he mentions, 'the "post-trial" point of view researchers ought to adopt when evaluating a test'. We do not accept it as given that researchers ought to adopt the post-trial point of view, and Spielman did not tell us why we should adopt it. In particular, it should be clear that Neyman and Pearson rejected the post-trial point of view. For example, in [1936*a*], p. 206 they say:

> In any given case it is impossible to determine whether a hypothesis is true or false, but it is possible to assess the efficiency of a statistical test by the manner in which it controls these two sources of error on repeated application in the situation defined by the mathematical model.

---

[1] Pearson continues in a footnote, 'This is perhaps the central problem over which opinions differ'. It certainly is one of the issues in Spielman's criticism.

[2] Spielman [p. 206] also notes that 'H. Robbins [1955] "An Empirical Bayes Approach to Statistics" [was] hailed by Neyman [1957; see also 1962] as "quite remarkable" and as the possible "forerunner of extensive important developments" . . .' Spielman uses this to support his claim that Bayesian methods are not foreign to NPT. Neyman complimented a colleague on a job well done; however, this hardly suffices to weave a new idea into an already existing body of thought.

(See also Pearson's remarks in Barnard, *et al.* [1962], p. 365.)

We have not found that Neyman or Pearson actually used the phrases 'pre-trial' and 'post-trial' point of view. However, it is clear from their exclusion of Bayesian philosophy from NPT, their explicit rejection of reasoning based directly on likelihoods (see *e.g.*, Pearson [1962], p. 277 and section 4 (*a*) *below*), and from statements such as that just quoted that they adopted the 'pre-trial' to the exclusion of the 'post-trial' point of view.

(*d*) *Size, Power, and the Neyman–Pearson Lemma*

After rejecting the Bayesian approach and the post-trial point of view, Neyman and Pearson continue in their first joint paper to identify two sources of error [1928*a*], p. 3:

> Setting aside the possibility that the sampling has not been random or that the population has changed during its course, Σ must either have been drawn randomly from Π, or from Π'. . . . The nature of the problem is such that it is impossible to find criteria which will distinguish exactly between these alternatives, and whatever method we adopt two sources of error must arise:
>
> (1) Sometimes, when Hypothesis H is rejected, Σ will in fact have been drawn from Π [Π being the population prescribed by H].
>
> (2) More often, in accepting Hypothesis H, Σ will really have been drawn from Π'.

Material in section 2 (*a*) *above* makes it clear that Neyman and Pearson were concerned with controlling these two sources of error, *i.e.*, with minimising the probabilities of the two kinds of errors. But it is equally clear that both kinds of errors cannot be simultaneously minimised (with a fixed number of observations). Neyman and Pearson therefore suggested the probability of a Type I error be fixed, and a test chosen to minimise the probability of a Type II error. Later, the term 'size of the test' was attached to the probability of rejecting H if it is true, *i.e.*, to the probability of a Type I error, and the term 'power of the test' was attached to the probability of rejecting H if it is false, *i.e.*, to one *minus* the probability of a Type II error.

Neyman and Pearson recognised early that the likelihood ratio[1] would be of great value in determining tests which are good in the sense of high power for fixed size. Their first five joint papers are concerned largely

---

[1] The *likelihood* of the outcome of an experiment is defined as any quantity proportional to the probability or probability density of that outcome; for example, if $x_1, x_2 \ldots x_n$ is a sample of n observations each drawn with a probability density function $f(x)$, then the likelihood of the sample is proportional to:

$$f(x_1)\,.\,f(x_2)\ldots f(x_n)$$

The *likelihood ratio* can therefore be described as the probability of the observed outcome under H divided by that probability assuming A.

with the formulation of well known statistical procedures as tests of a hypothesis H versus an alternative A, and showing that the tests based on the likelihood ratio are the known tests.

In working with these concepts, they began to appreciate even more their strengths and their interrelationship. In their sixth joint paper, they proved what is now known as the Neyman–Pearson lemma. In introducing their result, they comment [1933*a*], p. 143:

> In earlier papers we have suggested that the criterion appropriate for testing a given hypothesis could be obtained by applying the principle of likelihood. This principle was put forward on intuitive grounds after the consideration of a variety of simple cases. It was subsequently found to link together a great number of statistical tests already in use, besides suggesting certain new methods of attack. It was clear, however, in using it that we were still handling a tool not fully understood, and it is the purpose of the present investigation to widen, and we believe simplify, certain of the conceptions previously introduced.

They then proceed to prove the Neyman–Pearson lemma which states that in testing a simple hypothesis H against a simple alternative A, the likelihood ratio test provides the 'best' test possible, in the sense of maximum power for fixed size (and fixed number of observations).

The Neyman–Pearson lemma does not apply to composite hypotheses because different procedures may be prescribed for each of the simple hypotheses comprising a composite hypothesis. In this case the class of tests considered acceptable must often be restricted in some fashion. The search for appropriate restrictions and the results obtained from using them comprise a major portion of NPT as it is now known (see *e.g.*, Lehmann [1959]). Spielman discusses in an appendix an example involving a composite alternative, A. He begins by placing a prior distribution over A, thereby converting it into a test of a simple H against a simple A. He then applies the same analysis he applied to Neyman's TB example (discussed in section 3 *below*). Therefore, the frequentist procedures usually applied in tests of composite hypotheses were not discussed by Spielman and are not relevant to his objections.

The important point is that Neyman and Pearson found that the use of the concepts of size and power implies the use of likelihood ratios to obtain the 'best' test in their sense.

With this discussion as a basis, we point out in section 3 that every Neyman–Pearson test of a simple hypothesis against a simple alternative is also a Bayesian test in a sense compatible with the example discussed by Spielman. In section 4 we consider with Spielman how the approach changes if we allow the option of suspending judgment. In Graves [1976] we extend the present discussion to examples involving composite hypotheses.

3 THE EQUIVALENCE BETWEEN BAYESIAN AND NEYMAN–PEARSON TESTS

The purpose of simple hypothesis testing, in accordance with the concept of 'inductive behaviour', is to decide whether we should act as though a hypothesis H were true or false. Spielman feels that we will usually know or at least can approximate a prior probability p that H is true.

In this case, if $(1-p)$ is the prior probability of the simple alternative A, then a Bayesian test[1] can be formulated to accept H whenever the posterior probability of H given an observation x, $P(H|x) \geqslant 0{\cdot}5$. If $P(H|x) < 0{\cdot}5$ we decide to act as though H is false and A is true. (The critical probability 0·5, at which a decision is made can be determined arbitrarily. Depending on the empirical situation and the actions contemplated under the choices 'H is true' or 'H is false', we may wish to accept H whenever $P(H|x) \geqslant 0{\cdot}7$ or $P(H|x) \geqslant 0{\cdot}1$ *etc.* The following discussion generalises readily in this direction.)

The equivalence between Bayesian tests and NPT is most easily seen in the following form of Bayes' theorem:

$$\text{(posterior odds)} = \text{(likelihood ratio)} \times \text{(prior odds)} \qquad (1)$$

where the odds for the hypothesis H are the probability of H divided by the probability of the alternative A. In particular, if 0·5 is the critical level of the posterior probability of H, *i.e.*, the probability below which we act as though H were false, then the equivalent critical posterior odds are $0{\cdot}5/(1-0{\cdot}5) = 1$.

Bayes' theorem (1) can be interpreted as a relation between the prior odds, the critical posterior odds, and the critical likelihood ratio, L' (*i.e.*, that value for the likelihood ratio below which we reject H and accept A). If we choose 1·0 as the critical posterior odds, then (1) can be rewritten as

$$L' = 1/\text{(prior odds)} \qquad (2)$$

Expression (2) converts the prior odds into a critical value for the likelihood ratio L'. The Neyman–Pearson size for a Bayesian test is the probability of obtaining an outcome with likelihood ratio less than L' under H, $P(L<L'|H)$, and its power is the same probability under A, $P(L<L'|A)$. Since the Bayesian test is based on the likelihood ratio, it is the 'best' Neyman–Pearson test (of its size).

---

[1] We are aware of two different models of 'Bayesian Tests' which have been used to compare and contrast Bayesian and frequentist procedures. One relies on the concept of regions of 'highest posterior density' discussed by Lindley [1964] and Box and Tiao [1965], and more recently by Sinha [1971]. The other is the one used by Spielman. It is used by Jeffreys [1939], and Pearson [1962]; a recent textbook which describes it is DeGroot [1970]. The two models are equivalent in the case of testing a simple H against a simple A.

Conversely, a Neyman–Pearson 'best' test can be defined in terms of its critical likelihood ratio. This critical likelihood ratio can be converted via (2) into prior odds for a Bayesian test. Hence, for every 'best' Neyman–Pearson test of a simple hypothesis H against a simple alternative A, there is an equivalent Bayesian test which uses the same likelihood ratio in the same way to make the same choice between H and A (and *vice versa*).

Of course, our choice for critical posterior odds must consider the costs or utilities associated with the two different kinds of errors. The remarks of Pearson [1962], p. 277 quoted above apply here as well. The formal machinery of Bayesian decision theory may be somewhat extraneous if our information about the relative costs of different kinds of errors is qualitative rather than quantitative. In the absence of quantitative prior information regarding probabilities of hypotheses (and costs of errors), I as a decision maker should be allowed to use the equivalent although less formalistic Neyman–Pearson model if for me as for Pearson [1962], p. 278 'there are some fundamental parts of the mechanism of subjective probability theory which simply will not get into gear with the way I think.'

We now discuss Neyman's [1950] TB example and Spielman's reaction to it. We show that subject to the constraints of the problem, the Bayesian and likelihood approaches advocated by Spielman dictate precisely the 'best' Neyman–Pearson test.

(*a*) *Neyman's TB Example*

Let us consider Neyman's [1950] TB example discussed by Spielman. In screening patients for TB, Neyman assumes we would take five chest X-rays of each patient. Since X-ray is not a perfect detector of TB, we must consider the probabilities of a positive indication on each X-ray when the patient does and does not have TB. For the sake of illustration, Neyman [1950] assumes that the probability of a positive response in one X-ray is 0·60 if the patient has TB, and 0·01 if not (independent of the responses on other X-rays). The binomial probability of a positive response if $k = 0, 1, 2, 3, 4,$ or 5 of the five X-rays is given in Table 1.

Neyman suggests we give a negative diagnosis only if all 5 X-rays are negative (*i.e.*, $k = 0$). The size for this test (*i.e.*, the probability of rejecting H given H) is $P(k = 0|H) = 0{\cdot}010$ and the power is $P(k = 0|A) = 0{\cdot}951$, where H denotes the hypothesis that the patient has TB, and A the alternate that the patient does not.

Of course, other tests might be used depending on the desired size. Accordingly, Table 1 gives the size of the 'best' Neyman–Pearson test which rejects H if k or fewer positive X-rays are detected. Since there are only a finite number of possible outcomes, the size and the power can achieve only a finite number of different pairs of values. To each achievable pair of values for the size and the power, there exists a range of equivalent values for the critical likelihood ratio. This in turn by expression (2) gives a range of equivalent values for the prior odds, or the prior probability, of H assuming a decision is to be made on the basis of whether the posterior odds for H are greater than or less than one.

TABLE 1

EQUIVALENCE OF BAYESIAN AND NEYMAN–PEARSON ANALYSES IN TESTING FOR TB

Test Based on a Total of 5 X-Rays

	Number of X-Rays Diagnosed as TB					
k	0	1	2	3	...	5
$P(k\|H)$*	0·01024	0·0768	0·2304	0·346	...	0·0778
$P(k\|A)$*	0·951	0·048	0·001	0·00001	...	
Likelihood ratio L	0·01077	1·60	237	35,000	...	
Characteristics of the test rejecting H on k or fewer X-rays						
Size	0·010	0·087	0·317	0·663	...	1·00
Power	0·951	0·999	0·99999		...	
Range or equivalent prior odds for H:						
Less than or equal	92·9	0·625	0·00421	0·00028	...	
Greater than	0·625	0·00421	0·00028		...	
Range of equivalent prior probability of H:						
Less than or equal	0·989	0·385	0·00419	0·00028	...	
Greater than	0·385	0·00419	0·00028		...	

* H: Patient has TB, *i.e.*, probability of diagnosing TB in one X-ray is 0·6. A: Patient does not have TB, *i.e.*, probability of diagnosing TB in one X-ray is 0·01.

From Table 1 we see that if the prior probability that the patient has TB is 0·99, even if none of the five X-rays is diagnosed as indicating TB, the patient will still have a greater than even chance of having TB (*i.e.*, $P(H|x) > 0{\cdot}5$). That is, if we are 99 per cent certain that the patient has TB, five clean X-rays are not sufficient for us to decide that this must be the one person in a hundred who does not have TB. If the prior probability of TB is greater than 0·385 but less than or equal to 0·989, the Bayes' test is the test Neyman recommends. If the prior probability of TB for a given patient lies between 0·00419 and 0·385, and we wish to say that TB is present whenever its posterior probability is greater than 0·5, the test rejecting TB on zero or one positive X-ray should be employed. (A similar analysis could be made if we wished to diagnose TB whenever its posterior probability is greater than 0·1 instead of 0·5).

This discussion can be generalised to describe the equivalence between Bayesian and Neyman–Pearson tests of any simple H against a simple A.

(*b*) *Spielman's Objection to Neyman's TB Example*

Spielman concludes that Neyman's concept of 'inductive behaviour' requires tests which are 'reliable'; he *defines* 'reliability' as follows [p. 207]:

> The ***reliability index of test T for experimental outcomes of type E in reference class R*** is the proportion of those decisions based on outcomes like *E* that would be correct were each member of *R* actually treated by tests like *T*.

In other words, the ***reliability index*** is the posterior probability of a correct decision given the outcome E. This ***reliability index*** becomes Spielman's criterion for goodness of a test [p. 208]:

> Of two tests designed for the same experiment, one is ***better than*** the other if it is at least as reliable as the other for every possible experimental outcome and more reliable for at least one such outcome.

Neyman and Pearson's criteria for goodness of a test are size and power. Spielman's adoption of an alternative criterion constitutes a shift in philosophy away from NPT. His reliability index (and his subsequent advocacy of likelihood—see section 4 *below*) is consistent with 'the "post-trial" point of view' he claims researchers 'ought to adopt' [p. 202], but is hardly addressing NPT 'on its own terms'.

Having established his criterion for evaluating different tests, Spielman then proceeds to apply this criterion to Neyman's TB example [p. 208]:

> Let y stand for the percentage of moderate cases of *TB* among some large class of potential examinees. . . . Let z stand for the percentage of people in the class who do not have TB. . . . The reliability index of the test for such outcomes ($k=0$) is . . . $0{\cdot}951z : (0{\cdot}01y+0{\cdot}951z)$. . . . In general, unless the size of a test is zero (which it never is in real life), low size, with or without high power, does not guarantee that a test has high reliability for those outcome types that lead to rejection of the hypothesis tested.

We are baffled by Spielman's use of y as a percentage here; standard convention dictates the use of proportions rather than percentages as it tends to simplify the mathematics. Perhaps Spielman is trying to make some point about what is or is not a probability. Whether this is the case or not, and whether y and z are percentages, proportions or probabilities, the expression $0{\cdot}951z{:}(0{\cdot}01y+0{\cdot}95z)$ is clearly Bayes' formula applied to the calculation of reliability. This would not surprise us were it not for Spielman's assertion two pages earlier [p. 206] that in the argument to follow,

> . . . no explicit use of Bayes' formula is made, in order to avoid giving the false impression that the arguments presuppose anything foreign to NPT.[1]

[1] In a personal communication, Spielman [1976] remarks 'The fact that there is a parallel between the class ratio arithmetic I use here and Bayes' theorem does not imply that I am employing Bayesian arguments or that my critique is inherently Bayesian.' In our view, Bayes' formula is a theorem in mathematics, and class ratio arithmetic is one realisation of it.

Spielman seems concerned that someone will confuse the size of the test with the reliability. He continues [p. 208]:

> There is nothing paradoxical about this: The size of a test is the maximum relative frequency of erroneous rejections of the hypothesis tested among *all members of the reference class*, whereas the reliability of a negative diagnosis is determined by the relative frequency of erroneous rejections among *those members of the reference class for which a rejection will occur*. These values can be radically different.

This seems to be the basis upon which he concludes [p. 202]:

> . . . that the size and power of an NP test are not truly relevant indices of the degree of security one should have in using the test; that size and power are dangerously misleading concepts; that good "best" NP tests may lead to decisions that are clearly stupid in light of the outcome of the experiment and the hypothetical probability distributions that govern it. . . .

We find it difficult to interpret size and power in a misleading fashion. It appears that Spielman accomplished this by trying to interpret them as posterior probabilities (which they clearly are not). This is exemplified by his attempts to relate size and power to 'reliability'.

One of Spielman's major concerns is the avoidance of outcomes with low reliability. As long as the number of samples is fixed in advance of the experiment it is impossible to avoid such 'unreliable' outcomes. On this point, Spielman remarks [p. 215].

> If the nature of his [the experimenter's] problem makes sequential observation and decision-making impossible, he may have to either take more observations or give up if one of these outcomes results.

Indeed, if a decision must be made and equivocal outcomes (*i.e.*, outcomes with likelihood ratio or posterior odds close to 1) are of concern, then the equivocal outcomes must be included in the acceptance region for either H or A and the ***behaviour assigned to the decision*** 'accept H' or to 'reject H and accept A' ***must take account of this equivocation.***

Thus, in spite of Spielman's claims that he was refuting NPT 'on its own terms', it is clear that he rejected the frequentist basis of NPT, asserting that one 'ought' to adopt the post-trial point of view; he advocated the use of Bayes' theorem and later, failing that, the direct use of likelihoods; he then claimed that size and power are 'dangerously misleading' because they are not posterior probabilities. This hardly constitutes a refutation of NPT in our minds, and is certainly not 'on its own terms'.

Up to this point, we have assumed that the number of observations was outside of our control. Indeed, in screening for TB, it may be that five X-rays present five different views, and it does not make sense to take more or less than five. Ignoring this possibility, we shall now consider

TABLE 2

ANALYSIS OF ALTERNATIVE NUMBERS OF X-RAYS IN NEYMAN'S TB EXAMPLE

	Decision			Equivocal outcome			
						Probability	
Number of X-rays N	Reject H if $X \leqslant k$ Positive X-Rays k	Size $P[X \leqslant k \mid H:\theta = 0·60]$	Power $P[X \leqslant k \mid A:\theta = 0·01]$	x	Likelihood ratio	Under H $P[X = x \mid H]$	Under A $P[X = x \mid A]$
1	0	0·04	0·9900000	0	0·4040	0·4	0·99
				1	60	0·6	1·01
2	0	0·16	0·9801000	0	0·1632	0·16	0·9801
				1	24·24	0·48	0·0198
3	0	0·0640	0·9702990	0	0·0660	0·0640	0·9702990
				1	14·843	0·2880	0·0294030
4	0	0·0256	0·9605960	0	0·02665	0·0256	0·9605960
				1	3·958	0·1536	0·0388120
5	0	0·01024	0·9509900	0	0·01077	0·01024	0·9509900
	1	0·08704	0·9990199	1	0·1599	0·0768	0·0480299
				2	237·38	0·2304	0·009702
6	0	0·004096	0·9414811	1	0·6461	0·0368640	0·0570595
	1	0·04096	0·9985396	2	95·95	0·1382400	0·0014408
7	0	0·0016384	0·9321653	1	0·26104	0·172032	0·0659037
	1	0·0188416	0·9979690	2	38·77	0·0774144	0·0019970
8	1	0·0085197	0·9973099	1	0·1055687	0·0078643	0·745652
	2	0·0498074	0·9999461	2	15·66	0·0412877	0·0026362
9	1	0·0038011	0·9965643	1	0·0426	0·0035390	0·0830471
	2	0·0250348	0·9999197	2	6·328	0·0212337	0·0033554
10	1	0·0016777	0·9957338	2	2·557	0·106169	0·0041524
	2	0·0122946	0·9998862	3	379·9	0·0424673	0·001118
	3	0·0547619	0·9999980				

11	1	0·0007340	0·9948203	2	1·033	0·0051905	0·0050243
	2	0·0059245	0·9998446	3	153·4	0·0233570	0·001523
	3	0·0292815	0·9999969				
12	1	0·0003188	0·9938255	2	0·4174	0·0024914	0·0059689
	2	0·0028102	0·9997944	3	61·98	0·0124571	0·0002010
13	1	0·0001376	0·9927511	2	0·1686	0·0011777	0·0069836
	2	0·0013153	0·9997347	3	25·049	0·0064777	0·0002586
	3	0·0077930	0·9999933				
14	2	0·0006087	0·9996649	2	0·06814	0·0005496	0·0080661
	3	0·0039064	0·999908	3	10·1187	0·0032977	0·0003259
	4	0·0175095	0·9999998				
15	2	0·0002789	0·9995842	2	0·02753	0·0002537	0·0092140
	3	0·0019278	0·9999875	3	4·0885	0·0016489	0·0004033
	4	0·0093477	0·9999997				
16	2	0·0001267	0·9994921	3	1·6520	0·0008118	0·0004914
	3	0·0009358	0·9999835	4	245·8	0·0039572	0·0000161
	4	0·0048957	0·9999996				
17	2	0·0000571	0·9993878	3	0·6674	0·0003943	0·0005908
	3	0·0004514	0·9999994	4	99·52	0·0020700	0·0000208
	4	0·025214	0·9999786				
18	2	0·0000256	0·9992708	3	0·296	0·001892	0·0007018
	3	0·0002148	0·9999726	4	40·023	0·0010646	0·0000266
	4	0·0012794	0·9999992				
19	2	0·0000114	0·9991406	3	0·1090	0·0000899	0·0008250
	3	0·0001013	0·9999656	4	16·150	0·0005394	0·0000334
	4	0·0006407	0·9999990				
20	2	0·0000050	0·9909964	3	0·4402	0·0000423	0·0009610
	3	0·0000473	0·9999574	4	6·546	0·0002697	0·0000412
	4	0·0003170	0·9999986				

Source: NES (1950) Applied Math Series 6. Tables of the Binomial Probability Distribution.

taking more than five X-rays; in section 4 we shall assume sequential observation is possible.

(*c*) *Taking More X-rays*

We now generalise the TB screening example by assuming that we can select in advance the number of X-rays to be taken. Our purpose is to demonstrate that the probability of obtaining an equivocal outcome (*i.e.* with likelihood ratio close to one) is reduced but not eliminated in larger experiments.

Table 2 presents the size and power of the test rejecting H with k or fewer positive X-rays, for a total number of X-rays ranging from 1 to 20.

Each of the tests shown is 'best of its size' for that number of observations. In making a selection from among the tests shown, the frequentist might scan this list and choose the test with the smallest number of observations whose size was small enough and power large enough to satisfy requirements.

As stated above, if sequential observation is impossible, it is impossible completely to avoid equivocal outcomes, *i.e.*, outcomes with likelihood ratio or Bayesian posterior odds close to one. Table 2 identifies with each test presented the most equivocal outcomes and their likelihood ratios. Also presented are the probabilities of the outcomes under H and A. We note that as the size decreases and the power increases with the increasing number of observations, the probabilities of equivocal outcomes under both H and A decrease.

For further illustration, the computations of Table 1 were repeated for fourteen observations and are present in Table 3.

We do not know which test Professor Spielman would recommend, subject to the constraints that either H or A must be selected and that the number of observations must be specified in advance. However, we think his selection procedure would begin by converting the prior odds he claims he would have into a critical likelihood ratio using expression (2) *above*. We think he would then scan a list of 'best' Neyman–Pearson tests such as those presented in Table 2 and select one with the equivocal outcome assigned to the decision 'accept A' (or H, since it must be assigned to one of the two possible decisions), and such that the probability of this equivocal outcome (computed using the prior probabilities of H and A) is appropriately balanced with the cost of the number of samples. Subject to these constraints, we think Professor Spielman would arrive at essentially the same test as the frequentist.

In the next section, we discuss Spielman's suggested direct use of likelihoods. We note that Neyman and Pearson deliberately rejected likelihoods as the philosophical basis for testing. In this section we have seen that NPT and likelihood inference dictates the same test (of a simple *H* against a simple *A*) when the sample size is fixed. We shall see that this duality also applies to Waldian sequential analysis.

TABLE 3

EQUIVALENCE OF BAYESIAN AND NEYMAN–PEARSON ANALYSES IN TESTING FOR TB

Test Based on a Total of 14 X-Rays

	Number of X-rays diagnosed as TB				
k	0	1	2	3	. . .
$P(k\|H)$*	$2{\cdot}68 \times 10^{-6}$	$5{\cdot}64 \times 10^{-5}$	$5{\cdot}50 \times 10^{-4}$	0·00330	. . .
$P(k\|A)$*	0·8687	0·1229	0·00807	0·000326	. . .
Likelihood ratio L	$3{\cdot}09 \times 10^{-6}$	$4{\cdot}59 \times 10^{-4}$	0·0681	10·12	. . .
Characteristics of the test rejection H on K or fewer X-rays					
Size	$2{\cdot}68 \times 10^{-6}$	$5{\cdot}91 \times 10^{-5}$	$6{\cdot}09 \times 10^{-4}$	0·00391	. . .
Power	0·8687	0·9916	0·99966	0·99999	. . .
Range of equivalent prior odds for H:					
Less than or equal	324,000	2,179	14·68	0·0988	. . .
Greater than	2,179	14·68	0·0988	0·000665	. . .
Range of equivalent prior probability of H:					
Less than or equal	0·999997	0·9995	0·936	0·0899	
Greater than	0·9995	0·936	0·0899	0·00067	

* H: Patient has TB, *i.e.*, probability of diagnosing TB in one X-ray is 0·6. A: Patient does not have TB, *i.e.*, probability of diagnosing TB in one X-ray is 0·01.

4 LIKELIHOODS

Spielman [p. 216] sets down the procedure he recommends for dealing with testing situations:

> Ideally, observations should be taken one at a time or in small groups (in order to avoid unnecessary expense), and the following calculations performed: (*a*) of the likelihood for all the data so far obtained of both hypotheses; (*b*) if there is a well-defined, natural reference class for which trustworthy, informative bounds on truth-frequencies can be set, reliability indices using the likelihoods computed under (*a*). . . . If such a reference class is not available, or if it is desired to avoid basing a decision on such relatively subjective factors (as is typically the case in scientific research) use the likelihoods of (*a*) to compute relative success–failure ratios; then reject an hypothesis if and only if the relative success–failure ratio for its rejection is high enough. . . . My own feeling is that for scientific work relative success–failure ratios on the order of a thousand or more will be required.

He concludes [p. 217]:

> . . . decision-makers should use likelihoods opportunistically, not worry about pre-designating sample sizes and using sophisticated mathematical techniques, in short, use likelihoods in a common-sense way.[1]

We find these recommendations anti-formalistic and inconsistent with Spielman's criticism of the formal Neyman–Pearson approach. In subsection (*a*) *below* we point out that a number of authors have considered what Spielman recommends. However, many statisticians including Neyman and Pearson find this approach lacking in philosophical basis. We lay this aside in subsection (*b*) to consider the consequences of applying this procedure. In this case as well we find that the procedure Spielman recommends is precisely the procedure based on size and power.

(*a*) *The Likelihood Principles*

It can be said that the basis of Neyman–Pearson theory is the likelihood ratio principle (see *e.g.* Pearson [1966]) which says that the likelihood ratio is the appropriate criterion for defining the critical region of the test. The Neyman–Pearson lemma proves this for the case of testing a simple hypothesis against a simple alternative. Since choosing a test is equivalent to specifying a critical region (*i.e.*, a set of outcomes under which we reject H), the emphasis in the Neyman–Pearson approach is on determining a critical region.

This is contrasted with the *likelihood principle*[2] which says that the likelihood function is all that is required to interpret the outcome of an experiment. This is fundamentally different from Neyman–Pearson theory because for example, likelihood inference involving Bernoulli trials is the same whether r successes were observed in an experiment planned for n trials or n trials were required to obtain a predetermined r successes. The frequentist statistician would find only a formal similarity between these two situations because the random variable is r in the first case, and n in the second. This is a critical difference to a frequentist because the *critical*

---

[1] This is not the approach Spielman adopts for himself [p. 217]:

> The above recommendations are intended for those who find the conceptual framework of NPT congenial. Personally, I do not. . . . I find a credibilist Bayesian approach most congenial.

Regarding Spielman's suggestion that we 'not worry about . . . using sophisticated mathematical techniques', we feel compelled to remark that in some applications (although by no means in all) these techniques are absolutely essential to careful advance planning so the experiment can be performed in a reasonable period of time at a reasonable cost. Indeed, one advocate of the likelihood principle, Barnard [1962], p. 323, has remarked, '*Before* the experiment is carried out (*i.e.*, in the design problem) probabilities are relevant, *after* the experiment likelihoods are relevant'.

[2] This term has taken a rather restricted meaning among present day statisticians. Spielman's use of it ([p. 202] '. . . Neyman and Pearson's early likelihood principle . . .') is not consistent with current usage.

region of a test is defined in terms of the values of the random variable observed as the outcome of the experiment.

In Barnard and Cox [1962], p. 55–6 Pearson gives us his (and Neyman's) view of the likelihood principle (also in Pearson [1962], p. 277; see also Pearson and Kendall [1970], p. 465 and Neyman [1962]):

> We also considered how far inference and decisions could be based on the numerical values of the likelihood ratios. But while we first obtained the critical or rejection regions of our theory as the contours in the sample space on which the appropriate likelihood ratio was constant, we thought the meaning of a test was more easily grasped if expressed in terms of the integral of the Z probability density inside (or beyond) the boundary of the region, rather than in terms of the likelihood ratio on the boundary.

It is clear that Spielman advocates the likelihood principle, at least for testing if not for the other problems of statistics (*e.g.*, estimation). As an editorial aside, we note that the use of the likelihood function as the primary concept in statistics has been an idea surrounded with controversy since it was first suggested by Fisher (*e.g.*, [1922], [1925]). (See, however, Savage [1976].) Its principal advocates have included Barnard (*e.g.* [1949] and with others [1962]) and Birnbaum [1962], whose views on this subject have changed (see *e.g.* [1972]). More recently, Edwards has made a number of contributions (*e.g.* [1974*a*], [1974*b*]). Unfortunately, the issue is far from resolved. Efron [1976], p. 484, recently remarked:

> It is hard to accept Fisher's endorsement of the likelihood principle. Bad experience with estimating the mean vector of a multivariate normal distribution has destroyed confidence in the "invariance principle," but this principle is itself just a weak consequence of the strict likelihood principle. Stein [1962] has given a forceful counter-example. On the other hand, the likelihood principle is remarkably useful, or at least convenient, for dealing with problems in sequential analysis, and don't seem to lead to disasters when applied to standard situations involving just a few parameters.

Our position is adequately represented by the remark of Dempster [1969], p. 359: 'Unfortunately, the reasoning behind the direct interpretation of a likelihood function alone has not been fully explicated and remains somewhat obscure to most statisticians.'

(*b*) *Sequential Testing*

However, in the situation described by Spielman, the situation is the same as that previously discussed for tests based on a fixed number of observations: both approaches utilise the likelihood ratio in exactly the same way. The only difference lies in whether we use this procedure because it maximises the power for a fixed test size, or because we are concerned about the relative success-failure ratio of the observed outcome.

The extension of the fundamental Neyman–Pearson lemma to cover sequential analysis was due to Wald [1947]. In particular, Wald showed that a sequential test of a hypothesis H against an alternative A which maximised the power for a fixed test size was based on the likelihood ratio of H over A, say L,

- Accepting H if $L \geqslant B$
- Rejecting H (*i.e.*, accepting A) if $L \leqslant C$
- Drawing another sample if $C < L$ and $L < B$

for suitably chosen constants B and C. Wald also showed that if α is the size and $1-\beta$ is the power, then B and C are approximately given by[1]

$$B \doteq \frac{1-\alpha}{\beta} \qquad C \doteq \frac{\alpha}{1-\beta} \tag{3}$$

(Wald [1947], *e.g.*, p. 47).

Spielman [p. 216] applies these formulae to find:

> Thus for a test with the low size 0·01 and the high power of 0·95, Wald's approximate sequential ratio test rejects the hypothesis tested if the relative success–failure ratio for rejection is a mere 95 [$=1/C$], and accepts it if the success–failure ratio for acceptance is an even lower 19·9 [$=B$]. Thus error probabilities are also liable to be quite misleading in connection with Waldian sequential tests. . . .

To press our point of the equivalence between the two approaches to sequential testing, we note that the above two formulae give B and C from α and β. They can, however, be solved to give α and β from B and C; *e.g.*,

$$1-\beta = \frac{B-1}{B-C} \qquad \alpha = C(1-\beta) \tag{4}$$

Spielman gives us his accuracy requirements in saying [p. 216]: 'My own feeling is that for scientific work relative success-failure ratios on the order of a thousand or more will be required.' If we interpret this statement as meaning $B = 1{,}000$ and $C = 0{\cdot}001$, then expressions (4) tell us that Spielman wishes to work with tests of power not less than 0·999, and size not more than 0·001. From his remarks, we get the impression that Spielman was unaware of the fact that this kind of precision generally requires large samples, even when taken sequentially.

---

[1] Although Spielman cites Wald [1947], for some reason he cites Lehmann [1959], for these approximate formulae: 'Using the approximations of Lehmann [1959], for test of simple hypotheses Wald's theory recommends the test which, on our terminology, rejects the hypothesis tested if and only if the relative success–failure ratio for its rejection is greater than the pre-assigned power divided by the pre-assigned size . . .' [p. 216]. Wald developed these approximations, including them in his [1947].

But it is more important to our understanding of Spielman's argument to consider his claim of the misleading character of size and power. We find a very direct (if only approximate) connection between error probabilities and the bounds he wishes to use on the likelihood ratio. That is $\beta = 0{\cdot}05$ is roughly $1/19{\cdot}9$; $\alpha = 0{\cdot}01$ is roughly $1/95$; $\alpha = \beta = 0{\cdot}001$ are $1/1{,}000$.

Wald's original motivation in developing the 'sequential probability ratio test' was to save labour in certain testing situations by reducing the average number of samples required ([1947], p. 1): 'The sequential probability ratio test frequently results in a saving of about 50 per cent in the number of observations over the most efficient test procedure based on a fixed number of observations.' This is, of course, on the average. In some proportion of the cases, *more* samples would be required than under the fixed sample size procedure.

Spielman's motivation for suggesting sequential procedures, on the other hand, was to avoid what might be termed 'equivocal outcomes' *i.e.*, outcomes with likelihood ratio close to 1. However, in the 'pre-trial' point of view adopted by Neyman and Pearson this consideration is irrelevant to the evaluation of alternative tests.

5 SUMMARY

We have discussed Spielman's 'A Refutation of the Neyman–Pearson Theory of Testing'. We have found it lacking in two very general ways:

1. Spielman claims to have refuted NPT 'on its own terms'. However, the grounds Spielman chooses are Bayesianism and the likelihood principle, both of which Neyman and Pearson deliberately excluded from NPT.

2. Spielman does not come to grips with the duality between different theories of testing. We have examined all of the examples discussed by Spielman (in this paper and in Graves [1976]). In every case we get the impression that the procedure Spielman would employ *given the constraints of the problem* is precisely the procedure that Neyman and Pearson would recommend.

University of Wisconsin-Madison

REFERENCES

(Spielman's references are included in the following list, marked with '*'. We cite either directly or in quotation all but Gillies [1971] and Robbins [1963] which are included only for completeness. He cites of Neyman only [1950], an elementary textbook hardly devoted to NPT, and [1957], which treats only one philosophical portion of NPT. He dismisses 'the early joint papers of Neyman and Pearson' as being 'mostly concerned with the mathematical aspects of the theory . . .'; we suspect he also read very little of Lehmann [1959], considered by many as the current authority on NPT, as it is even more mathematical than the early joint papers of Neyman and Pearson.)

P1093

BARNARD, G. A. [1949]: 'Statistical Inference', (with discussion), *Journal of the Royal Statistical Society, Series B*, **11**, pp. 115–49.

* BARNARD, G. A. and COX, D. R. (*eds.*) [1962]: *Statistical Inference.*

BARNARD, G. A., JENKINS, G. M. and WINSTON, C. B. [1962]: 'Likelihood Inference and Time Series' (with discussion), *Journal of the Royal Society, Series A*, **125**, pp. 321–72.

BIRNBAUM, A. [1962]: 'On the Foundation of Statistical Inference', *JASA*, **57**, pp. 269–326.

BIRNBAUM, A. [1972]: 'More on concepts of statistical evidence', *JASA*, **67**, pp. 858–61.

BOX, G. E. P. and TIAO, G. C. A. [1965]: 'Multiparameter problems from a Bayesian point of view', *Annals of Mathematical Statistics*, **36**, pp. 1468–82.

DEMPSTER, A. P. [1969]: *Elements of Continuous Multivariate Analysis.*

DEGROOT, M. H. [1970]: *Optimal Statistical Decisions.*

EDWARDS, A. W. F. [1974*a*]: 'The History of Likelihood', *Int. Stat. Rev.*, **42**, pp. 9–15.

EDWARDS, A. W. F. [1974*b*]: *Likelihood.*

* EDWARDS, W., LINDEMAN, H. and SAVAGE, L. J. [1963]: 'Bayesian statistical inference for psychological research', *Psychological Review*, **70**, pp. 193–242.

EFRON, B. [1976]: 'Discussion of L. J. Savage', [1976]: *Annals of Statistics*, **4**, pp. 483–4.

* GILLIES, D. A. [1971]: 'A falsifying rule for probability statements', *British Journal for the Philosophy of Science*, **22**, pp. 231–61.

GRAVES, S. B. [1976]: *A Frequentist Response to Some Bayesian Criticism*, unpublished manuscript.

* HACKING, I. [1965]: *Logic of Statistical Inference.*

* HODGES, J. L. and LEHMANN, E. L. [1964]: *Basic Concepts in Probability and Statistics.*

JEFFREYS, H. [1939]: *Theory of Probability*, corrected third edition [1967].

* LEHMANN, E. L. [1959]: *Testing Statistical Hypotheses.*

LINDLEY, D. V. [1964]: 'The Bayesian analysis of contingency tables', *Annals of Mathematical Statistics*, **35**, pp. 1622–43.

NATIONAL BUREAU OF STANDARDS [1950]: *Applied Math Series 6: Tables of the Binomial Probability Distribution.*

* NEYMAN, J. [1950]: *A First Course in Probability and Statistics.*

* NEYMAN, J. [1957]: ' "Inductive Behavior" as a basic concept in the philosophy of science', *Review of the International Statistical Institute*, **25**, pp. 7–22.

NEYMAN, J. [1962]: 'Two breakthroughs in the theory of statistical decision making', *Review of the International Statistical Institute*, **30**, pp. 11–27.

NEYMAN, J. and PEARSON, E. S. [1928*a* and *b*]: 'On the use and interpretation of certain test criteria for purposes of statistical inference', Parts I and II, *Biometrika*, **20A**, pp. 175–240 and pp. 263–94. Reprinted in Neyman and Pearson [1966].

NEYMAN, J. and PEARSON, E. S. [1933*a*]: 'On the problem of the most efficient tests of statistical hypotheses', *Phil. Trans. Roy. Soc. A*, **231**, pp. 289–337. Reprinted in Neyman and Pearson [1966].

NEYMAN, J. and PEARSON, E. S. [1933*b*]: 'The testing of statistical hypothesis in relation to probabilities *a priori*', *Proc. Camb. Phil. Soc.*, **24**, pp. 492–510. Reprinted in Neyman and Pearson [1966].

NEYMAN, J. and PEARSON, E. S. [1963]: 'Contributions to the theory of testing statistical hypotheses', *Statistical Research Memoirs*, **1**, pp. 1–37. Reprinted in Neyman and Pearson [1966].

NEYMAN, J. and PEARSON, E. S. [1966]: *Joint Statistical Papers of J. Neyman and E. S. Pearson.*

PEARSON, E. S. [1962]: 'Some thoughts of statistical inference', *Annals of Mathematical Statistics*, **33**, pp. 394–403. Reprinted in Pearson [1966].

PEARSON, E. S. [1966]: *The selected papers of E. S. Pearson.*

PEARSON, E. S. [1967]: Discussion on D. J. Bartholomew 'Hypothesis testing when the sample size is a random variable', *Journal of the Royal Statistical Society*, Series B, **29**, pp. 53–82.

PEARSON, E. S. [1966]: 'The Neyman Pearson Story, 1926–1934', in F. N. David (*ed.*) *Festerchrift for J. Neyman*, pp. 1–23. Reprinted in Pearson and Kendall [1970], pp. 455–77.

* PEARSON, E. S. and KENDALL, M. G. (*eds.*) [1970]: *Studies in the History of Statistics and Probability.*

* ROBBINS, H. [1955]: 'An empirical Bayes approach to statistics', in J. Neyman (*ed.*). *Proceedings of the Third Berkeley Symposium on Mathematical Probability and Statistics.*

* ROBBINS, H. [1963]: 'A new approach to a classical statistical decision problem', in H. Kyburg and E. Nogel (*eds.*): *Induction: Some Current Issues*, pp. 101–10.

SAVAGE, L. J. [1976]: 'On rereading R. A. Fisher' (with discussion), *Annals of Statistics*, **4**, pp. 441–500.

SINHA, B. K. [1971]: 'On the equivalence of Bayesian and non-Bayesian procedures', *Calcutta Statistic Assn. Bull.*, **20**, pp. 1–22.

SPIELMAN, STEPHEN [1973]: 'A refutation of the Neyman–Pearson theory of testing', *British Journal for the Philosophy of Science*, **24**, pp. 201–22.

SPIELMAN, STEPHEN [1976]: personal correspondence.

STEIN, C. [1962]: A remark on the likelihood principle, *Journal of the Royal Statistical Society*, Series A, **125**, pp. 565–73.

* WALD, A. [1947]: *Sequential Analysis*, Dover reprint [1973].

Brit. J. Phil. Sci. 29 (1978), 25–38 *Printed in Great Britain*

The Interpretation and Justification of the Subjective Bayesian Approach to Statistical Inference*

by RICHARD BUXTON

Introduction.

1 *The Subjective Bayesian Approach.*

2 *Difficulties with the Bayesian Approach.*

3 *Can the Models M_i be given an Empirical Interpretation?*

4 *The Nature of a Mathematical Model.*

5 *Reformulation of the Bayesian Argument.*

6 *Does the Reformulated Argument Support Current Bayesian Practice?*

7 *An Alternative Interpretation.*

8 *Criticism of the Alternative Interpretation.*

Conclusion.

INTRODUCTION

This paper examines the interpretation of the subjective Bayesian approach to statistical inference.

In section 1, I outline the structure of the Bayesian approach. In sections 2 and 3, I argue that it is difficult to assign degrees of belief to probability models, as seems to be required in a subjective Bayesian argument. I then propose two ways out of this difficulty, and examine their implications for current Bayesian practice:

(*i*) First, I suggest that we should link our models, which are to be regarded as models for our degrees of belief, to subsets of the set of all possible data sets that could arise from the cases in which we envisage making predictions. Degrees of belief are then assigned to the subsets (sections 4–6).

(*ii*) In view of some difficulties with interpretation (*i*), I suggest an alternative approach in which we regard the 'probabilities' that we assign to the models as in themselves uninterpretable, but nevertheless capable of giving rise to a predictive distribution that can be interpreted in a degree of belief sense (sections 7 and 8).

---

*I am grateful to Dr Donald Gillies, Professor Dennis Lindley, and the referees of the *British Journal for the Philosophy of Science*, for their comments on an earlier draft of this paper.

Received 28 *January* 1977

1 THE SUBJECTIVE BAYESIAN APPROACH

In this paper, we attempt to explicate the Bayesian argument as it is used in practical decision making. We assume that the aim of statistical inference is to derive a predictive distribution of degrees of belief over a set of possible outcomes. For example, we may wish to obtain a distribution over the number of defective components in a batch of items coming off a production line, or over the possible levels of air traffic in 1980; once we have our distribution, we can use it to help us to decide whether to accept or reject a batch, or to help us to choose between several different schemes for developing airport provision.[1]

By the 'subjective Bayesian approach', we mean an approach that proceeds along the following lines. We have a set of models (hypotheses) M_i, each model specifying a probability distribution over the possible sets of data. We express our degrees of belief in the models by assigning a prior probability distribution over them. We subsequently update this distribution, in the light of any data we collect, by using Bayes' theorem:

$$P(M_i \mid \text{data}) = \frac{P(\text{data} \mid M_i) \times P(M_i)}{P(\text{data})}$$

At any stage, we can obtain a predictive distribution over a set of outcomes O_j by using

$$P(O_j) = \sum_i P(M_i \mid \text{data received so far}) \times P(O_j \mid M_i)$$

(Note: The $\sum$ sign is intended to denote either summation or integration, as appropriate.)

In the following argument, I have assumed that degrees of belief are adequately explicated by betting quotients. However, the argument is unaffected by the adoption of an alternative explication such as de Finetti's approach through losses (de Finetti [1975], ch. 3).

2 DIFFICULTIES WITH THE BAYESIAN APPROACH

Although the approach to inference that we have just outlined is mathematically simple, its interpretation and logical justification is, I believe, obscure. In particular, most presentations of the argument fail to make clear the nature of the models M_i over which we are asked to assign a prior probability distribution. When I assess my degree of belief in a particular M_i, is M_i supposed to be simply a model for my degrees of belief (in given possible sets of data), or is it supposed to describe some features of the world—that is, to be a model of empirical probabilities?

---

[1] The difficulties that are discussed in sections 2 and 3 apply equally to the application of the Bayesian argument in scientific inference. In science, however, we may be interested not so much in obtaining a predictive distribution as in assigning a measure of plausibility to each of a set of hypotheses. If we are interested in the hypotheses for their own sake, rather than simply as instruments in obtaining a predictive distribution, the explication suggested in sections 7 and 8 may seem unsatisfactory. With this proviso, I believe that the arguments of sections 4 to 8 could be reworked in the context of scientific inference.

We can quickly dismiss the idea that the M_i could be simply models for degrees of belief. If they were, we should, in effect, be assigning degrees of belief to degrees of belief and this leads to contradiction. For example, suppose we wish to assess a predictive distribution over the quality (OK/defective) of an item that we are going to pick from a production line. We must first select a class of models, M_i, each model representing a set of degrees of belief that we think we might give if asked to assess degrees of belief over different sets of data. In this example, we might choose each M_i to be the Binomial model with a given probability parameter. Notice that, since we are required to assess a probability distribution over the M_i, these models must exhaust all possible degree of belief distributions that we would consider giving. Now suppose that, for some reason, we would consider only two possible models—M_1 (Binomial, probability parameter for a defective $= \frac{1}{2}$) and M_2 (Binomial, probability parameter $= \frac{3}{4}$). For each M_i, we have to assess our degree of belief that, if asked, we would assign our degrees of belief over possible sets of data in accordance with the prescriptions of M_i. Suppose we assign probabilities p_1 to M_1 and p_2 to M_2. Using the multiplication law, our predictive distribution becomes

$$P(\text{defective}) = (p_1 \times \tfrac{1}{2}) + (p_2 \times \tfrac{3}{4})$$

Except in the degenerate cases where p_1 or $p_2 = 0$, we have an assignment of degrees of belief that does not conform to either of our two models M_1, M_2. And yet we have had to assume that these are the only two possible models for our degrees of belief.

Having dismissed the idea that the M_i are simply models for our degrees of belief, we must presumably try to given them some empirical interpretation. It is important, however, to note that the interpretation of the models M_i as statements about the world does not prevent us from, at the same time, using the M_i as models for our degrees of belief. We may well feel that if M_i is an appropriate description of the world, then it will also be an appropriate model for our degrees of belief. So although we have avoided the problem of assigning a probability distribution directly over a set of models representing degrees of belief, we may still be able to give the probabilities in our model—the $P(\text{data} \mid M_i)$—a 'degree of belief' interpretation. This is vital since, unless it is possible to give a 'degree of belief' interpretation to *all* the probabilities in our applications of the Addition/Multiplication rules and Bayes' theorem, these rules cannot be justified by the coherence requirement.

3 CAN THE MODELS, M_i, BE GIVEN AN EMPIRICAL INTERPRETATION?

We now attempt to give the models, M_i, an empirical interpretation in such a way that we can meaningfully assign degrees of belief to them. In order to assess a degree of belief distribution and to carry through the Bayesian argument outlined in section 1, we shall need to assume that one, and only one, of the models is 'true' (that is, the models are exhaustive and mutually exclusive), and that we could, in principle at least, determine which of the M_i is 'true'.

Imagine, first, a situation in which we have a machine that will generate data in accordance with any one of a given class of probability models, M_i. The machine has a knob which we can use to select any of the available models. A friend sets the knob and we observe the data generated. Clearly, in this situation, there is no problem with assigning degrees of belief over the M_i. We know that the machine is such that it can only produce data in accordance with one or other of the given class of models, and we could determine which was the true model by asking our friend to show us the position of the knob on the machine.

In most applications of statistics, the situation is not so simple. For example, suppose we are trying to predict the crushing strength, y, of a specimen of concrete in terms of the number of days, x, since it was mixed. We may choose to use simple linear models of the form $y_i = a+bx_i+\epsilon_i$, where the ϵ_i's are independent, have zero means and constant variance σ^2, and follow a Normal distribution. But we do not seriously believe that y_i values are generated by feeding the x_i into a machine with the parameters a, b, σ set at some particular values. What then could we mean by asserting that a particular model M_i is 'true'? We might, rather informally, say that what we mean is that the y_i values behave *as if* they were generated by a machine set to generate data in accordance with the model M_i. But how can we ever *know* whether or not the data are behaving as if they were generated by a machine set at M_i?

It is as if we were back in the "machine" situation, but with the knob positions unlabelled. We might, in such a case, attempt to deduce the setting of the machine by examining a long run of data. We expect that, if we run the machine for a very long time, the distribution of the resulting data will approximate closely to that described by the appropriate model. There is, of course, some vagueness here since, in a finite run of the machine, the distribution of the data is unlikely to conform precisely to the prescriptions of the model. However, provided that the run of data is long enough, the vagueness may be negligible.

This reasoning suggests that, in the more usual non-machine situation, we could define the 'true' model as the model to which a long run of data would conform. In order for the vagueness in this definition to be unimportant, we must be able, in principle at least, to collect a large amount of data. In many scientific applications of statistics, this is quite possible—in the example on the crushing strength of concrete, we could, in principle, make and test an almost limitless number of concrete specimens. On the other hand, in many economic and social applications, we are observing the real world as it is, rather than experimenting with it. Even given limitless time, it may be impossible to collect the data that we would need in order to make the vagueness in our definition negligible.

We have noted that it is often impossible to give a precise *operational* meaning to the notion 'M_i is a true model'. But proponents of a *propensity* interpretation of probability would claim that the notion 'M_i is true' is nonetheless quite meaningful, even in cases where the amount of data that we can collect is strictly limited. Although the adoption of this point of view might allow us to assign degrees of belief to individual models, it would still not enable us to carry through the argument outlined in section 1, since there is no reason to suppose that the models that we are currently considering are exhaustive, or even mutually exclusive. Of course, we often have some *a priori* reason for selecting a particular class of models, but the range of possible models is enormous, and we can never be sure that our class of models contains a 'true' one. On the other hand, we could quite easily have *several* 'true' models. For example, suppose we were trying to build a model relating a quantity y to two mutually independent quantities x_1 and x_2. There could be two 'true' models, one of the form $y = a+bx_1+\epsilon$, and the other of the form $y = a+bx_1+cx_2+\epsilon$, where ϵ is a random error term with some specified distribution. The second model might well be preferable, since the inclusion of a term in x_2 would probably reduce the size of the unexplained variation assigned to the error term ϵ, but from a propensity point of view, the two models could both be 'true'.

4 THE NATURE OF A MATHEMATICAL MODEL

We have seen that there are difficulties both in giving a meaning to the notion of a 'true' model, and also in using this idea in a Bayesian argument. We could try to overcome these difficulties in either of the following ways: (1) by retaining the notion of a 'true' model, but modifying the Bayesian argument, or (2) by abandoning the notion of a 'true' model. The second of these approaches seems to me to be more in keeping with the way in which models are used in statistical practice.[1]

When we use a deterministic mathematical model, we do not expect that the real world will behave in perfect conformity with the prescriptions of the model. The model is simply regarded as an approximate description of the real situation. Surely, the same is true of probability models. For example, when we adopted the class of simple linear models to describe the relationship between the crushing strength of concrete and the number of days after mixing, we were not saying that, if we took enough specimens, the mean crushing strengths for the different numbers of days after mixing would lie on a perfect straight line, and that, for any value of 'days after mixing', the values for 'crushing strength' would be precisely Normally distributed around the line. The fact that we adopted the given class of models does, of course, suggest that we would expect the means to lie *roughly* on a straight line, though we are not ruling out the possibility of some curvature. Similarly, for any value of 'days after mixing' we would expect the distribution of 'crushing strength' to be roughly Normal, but we are not ruling out some slight departures from Normality. So although we consider it most unlikely that long run data would conform precisely to *any* of the models we are considering, we feel that at least one of these models is likely to provide a good enough approximation to reality for us sensibly to use it to describe our degrees of belief. As an indication that this view of models is accepted by practising statisticians, see Nelder [1977].

Notice, incidentally, that there is no contradiction in accepting that there may be discrepancies (in a frequency sense) between the real situation and the model and, at the same time, using the model to describe one's degrees of belief. As an illustration, take the example on the crushing strength of concrete. We are quite free to use a Normal model to describe our degrees of belief in different variations around the regression line, whilst accepting that the long run distribution of the data might well differ from Normality. The point is that, in the absence of information indicating specific types of departure from Normality, the Normal model seems a reasonable one for our degrees of belief.

5 REFORMULATION OF THE BAYESIAN ARGUMENT

We are now saying that we wish to use the model M_i not just when M_i is 'true' (in a long run frequency sense), but in any situation in which we consider M_i to be a reasonable approximation. So surely we should be trying to assign a prior probability distribution not over the events 'M_i is true', but over the events 'M_i is an adequate approximation'.

---

[1] This suggestion has also been made by Leamer [1972] who has argued that we need a method of drawing inferences based on false models.

If we are to assign probabilities to the events 'M_i is an adequate approximation', we must refine the concept 'adequate approximation'. To see the need to do this, notice that whilst we presumably expect at least one of our models to be adequate, there seems no reason why there should not be several adequate models. In other words, the events 'M_i is an adequate approximation' are not necessarily mutually exclusive.

We proceed as follows. We first consider the set of all forseeable cases in which we expect to use the prediction system that we are developing. We include any cases that we use to calibrate (update) the system. Taking the 'crushing strength of concrete' example, we would consider all possible specimens of concrete to which we envisage applying our prediction system. Notice that there is no requirement that the set of cases should be large, so we can apply this procedure in economic/social applications of statistics where the number of conceivable predictions is usually strictly limited. It is perhaps worth remarking that, in de Finnetti's terms, we are trying to construct a set of exchangeable events.

We now consider the outcomes of all the cases we are considering. We try to partition the set of all *possible* data sets (collections of outcomes) into disjoint subsets S_i, in such a way that we can establish a 1–1 relationship between the subsets S_i and the models M_i. Intuitively, we want S_i to be the subset containing those data sets for which M_i would be the most sensible model.

Let us consider how we might apply this procedure to the 'crushing strength of concrete' example. A data set will consist of a collection of (x, y) pairs, each pair describing a particular case in which we make a prediction. We now have to split the set of all such collections into subsets that can be put into a 1–1 relation with the linear models that we are considering. Take the model M_i with parameters a_i, b_i, σ_i. One way of defining a subset S_i would be 'the set of all collections of (x, y) pairs for which the least squares line is $y = a_i + b_i x$ and the variance of deviations from this line is σ_i^2'. (Notice that the 'least squares line' here is fitted to the set consisting of all the data from forseeable predictions. It is not the least squares line through some data that we are using to calibrate the model.) Of course, this way of defining the subsets S_i will only be sensible if we feel that each model M_i would provide an adequate representation of our degrees of belief for any likely data set in the corresponding subset S_i.

I now digress to dispose of an apparent weakness with the procedure I am suggesting. Each subset S_i will contain *some* data sets for which none of the proposed linear models would be appropriate. For example, the data may cluster around a curve rather than a straight line. Surely it is unsatisfactory that, in such a case, our procedure is still suggesting that we use a linear model for our degrees of belief. However, at the time we assign our degrees of belief, we feel that the data will cluster roughly around a straight line. (Otherwise, we ought to be considering some curvilinear models.) So, to say that a data set is in S_i, is taken to imply that the data is clustered roughly around the corresponding straight line, and hence that the implied linear model M_i is sensible as a description of our degrees of belief. If, after collecting and examining some data, we are led to suspect that a data set in S_i is likely to be clustered around a curve rather than a straight line, then the use of linear models for our degrees of belief will no longer seem sensible, and a predictive distribution yielded by using these models will no longer be binding.

6 DOES THE REFORMULATED ARGUMENT SUPPORT CURRENT BAYESIAN PRACTICE?

I have outlined one possible interpretation of the Bayesian approach. We now ask whether this interpretation supports current Bayesian practice. Unfortunately, it does not. There are two main problems:

(*i*) Since we are now working in terms of collections of possible outcomes over a *finite* number of cases, the independence assumptions made in many of the usual statistical models no longer seem justified.

For example, suppose we are in a simple regression situation, and are considering how we ought to assign our degrees of belief given that the least squares line through the data of all foreseeable cases is $y = a+bx$ with the variance σ^2. Take a *model* $y = a+bx+\epsilon$ with the usual assumption that the errors are independent. Clearly, many of the data sets that would be compatible with this model will be ruled out because $y = a+bx$ is not the least squares line through the data points.

The effect of using models that assume independence will of course be particularly serious in situations where the number of foreseeable cases we are considering is small, as for example in econometrics. Here, we shall often be making predictions that almost exhaust the set of forseeable cases. So the predictions will be much more severely constrained than the usual models would suggest.

(*ii*) The second difficulty with our interpretation is that it does not support the use of the Bayesian argument to introduce models that were not among our original set $\{M_i\}$. We look at two examples.

First, we take the example on the crushing strength of concrete. But we now extend our class of models to include not just simple linear models, but also exponential models of the form $y_i = a \,.\, b^{x_i} + \epsilon_i$, where the ϵ_i have the same properties as in the case of the linear models. (We presumably believe, for some reason, that the relationship between 'crushing strength' and 'days after mixing' is likely to conform roughly to either a straight line or an exponential curve.) By analogy with our procedure in the 'linear models' case, we partition the set of possible data sets as follows. We associate with a model M_i the subset consisting of those data sets for which the straight line/exponential curve corresponding to M_i provides the least squares fit. (There is a difficulty here in that we could have a data set for which there was a joint least squares fit—the least squares line and the least squares curve being equally adequate/inadequate. However, our assumption that the data set will cluster around either a straight line or an exponential curve implies that we consider it unlikely that we shall have a joint least squares fit).

Now suppose that, if we could see the outcomes of all the situations in which we make predictions, the resulting data set would cluster around a smooth curve intermediate between a straight line and an exponential curve. As we collect more and more data, and input it to the Bayesian 'machine', the probability distribution over the models will not tend towards 1 on a single model and 0 on the rest. Instead the probability will tend to become equally distributed between appropriate straight line and exponential models. So the predictive distribution will be a compromise between a straight line and an exponential model—in particular, it will reflect a curvature intermediate between the two.

It appears then, that the Bayesian argument has introduced a model that we had not thought of, but that is more sensible than any of the models we chose to consider.[1] We shall now examine the logic of this intuitively desirable property of the Bayesian approach.

Suppose we have arrived at the situation mentioned above, where we have obtained a considerable amount of data and have a predictive distribution which is a compromise between a straight line and an exponential model. The fact that our current probability distribution over the models assigns a substantial amount of probability to both straight line and exponential models, may cause us to question our original formulation of the problem, and to conclude that we should consider models other than straight line/exponential ones. But once we have accepted this, the logical justification of our predictive distribution collapses. If we were now told that the data set of predictions was in a subset S_i, it would no longer seem reasonable to assume that the data were clustered around the straight line/exponential curve corresponding to the associated model M_i. So it would not be at all obvious that the model M_i would be appropriate to describe our degrees of belief. The argument leading to the predictive distribution now has a missing link—the $P(\text{data}|M_i)$ do not represent our degrees of belief, and so we are no longer compelled to assign degrees of belief in accordance with the predictive distribution. (Note: This is just one instance of the general problem of new information causing us to abandon some of the degrees of belief that we fed into the Bayesian argument, and hence making the conclusions of the argument no longer binding. This problem has been discussed by a number of writers—see, for example, Hacking [1967].)

---

[1] The use of the Bayesian argument to 'home-in' to a suitable *mixture* of models, has been advocated by a number of Bayesian statisticians—see, for example, Harrison and Stevens [1976].

Before leaving this first example, we should note that the argument we have been criticising may, of course, lead to intuitively sensible results. We have simply shown that it cannot be logically justified by the coherence requirement.

For our second example, we shall imagine that we have a consignment of components, and we are trying to obtain a predictive distribution for the quality (OK/defective) of a single item.

In this problem, a data set of possible outcomes might consist of a specification of the state of each item in the consignment. We might decide to use Binomial models to represent our degrees of belief, and to link the models to the members of our set of data sets in the following way. Writing M_p for the set of Binomial models with probability parameter p, we shall associate M_p with the subset S_p consisting of all data sets for which the proportion of defectives is p.

However, let us suppose that, initially, we believe that the proportion of defectives in the consignment will be either close to $\frac{1}{2}$ or close to $\frac{3}{4}$. If the proportion were around $\frac{1}{2}$, we would consider the Binomial models, $M_{\frac{1}{2}}$, to provide a sensible model for our degrees of belief. If the proportion were around $\frac{3}{4}$, then $M_{\frac{3}{4}}$ would be an appropriate model. We relate these two models to the possible states of the consignment in the following way. Writing P for the proportion of defectives, we decide to use $M_{\frac{1}{2}}$ if $|P-\frac{1}{2}|$ is smaller than $|P-\frac{3}{4}|$, and use $M_{\frac{3}{4}}$ if $|P-\frac{3}{4}|$ is smaller than $|P-\frac{1}{2}|$. Given our assumption that the proportion of defectives will be close to $\frac{1}{2}$ or $\frac{3}{4}$, we ignore the possibility of obtaining $\frac{5}{8}$, and hence of $|P-\frac{1}{2}|$ and $|P-\frac{3}{4}|$ being equal.

Suppose now that the proportion of defectives in the consignment is neither close to $\frac{1}{2}$ or $\frac{3}{4}$. Let us assume that it is in fact $\frac{5}{8}$. As we observe more and more items from the consignment, the probabilities on the two possibilities we are considering ('proportion defective around $\frac{1}{2}$', and 'proportion defective around $\frac{3}{4}$') will tend towards $\frac{1}{2}$. So the predictive distribution for a single item will tend towards

$$P(\text{defective}) = (\tfrac{1}{2}\times\tfrac{1}{2})+(\tfrac{1}{2}\times\tfrac{3}{4}) = \tfrac{5}{8}.$$

Notice that if we had considered a wider class of models including $M_{\frac{5}{8}}$ (to be used when the proportion of defectives is around $\frac{5}{8}$), we would, after observing a large number of items, have arrived at a similar conclusion. In this case, the probability on 'proportion around $\frac{5}{8}$' would have tended to 1, and the probabilities on the other possible outcomes could have tended to zero. So the predictive distribution would have approached

$$P(\text{defective}) = (1\times\tfrac{5}{8}) = \tfrac{5}{8}.$$

Although these two arguments lead to the same predictive distribution, there is an important difference between them. The first argument leads us to the conclusion that the original formulation on which it was based is inadequate—the models used for our degrees of belief no longer seem reasonable. (For example, if we are told that $|P-\frac{1}{2}|$ is less than $|P-\frac{3}{4}|$ we shall not conclude that the proportion of defectives is around $\frac{1}{2}$. Any value between 0 and $\frac{5}{8}$ is now quite conceivable. So the model $M_{\frac{1}{2}}$ is no longer a sensible representation of our degrees of belief.) The second argument, on the other hand, considers the possibility 'proportion defective around $\frac{5}{8}$' from the start, and so its logic is unaffected by its conclusion.

7 AN ALTERNATIVE INTERPRETATION

In view of the difficulties discussed in section **6**, we now attempt to give an alternative interpretation of the Bayesian approach.

We have seen that the difficulty in interpreting the Bayesian argument arises from the apparent need to assign a probability distribution over a set of models. We might then think that we could avoid the difficulty by regarding the prior probabilities that we feed into the Bayesian 'machine' as, in themselves, uninterpretable, but nevertheless capable of giving rise to a predictive distribution that can be interpreted in a degree of belief sense.[1]

Imagine first that we are in the situation where our data is produced by a machine set to generate data in accordance with one of the models M_i. We wish to develop a predictive system which we shall apply in a number of cases. We denote the possible collections of outcomes by C_1, C_2, C_3, . . . *etc.*

---

[1] This view seems to be suggested by de Finetti [1975], chapter 11, and by several other writers on subjective Bayesianism—see, for example, Hill [1972].

We can now proceed to apply a Bayesian argument in either of the following ways:

(*i*) We assign prior probabilities to the models M_i, and subsequently update these probabilities using Bayes' Theorem:

$$P(M_i \mid \text{data}) = \frac{P(\text{data} \mid M_i) \times P(M_i)}{P(\text{data})}$$

At any stage, we can obtain a predictive distribution over a set of outcomes O_j by using

$$P(O_j) = \sum_i P(M_i \mid \text{data received so far}) \times P(O_j \mid M_i)$$

or

(*ii*) We assign prior probabilities to the collections of outcomes, C_k, and subsequently updata these using Bayes' Theorem:

$$P(C_k \mid \text{data}) = \frac{P(\text{data} \mid C_k) \times P(C_k)}{P(\text{data})}$$

At any stage we can obtain a predictive distribution over a set of outcomes O_j by using

$$P(O_j) = \sum_k P(C_k \mid \text{data received so far}) \times P(O_j \mid C_k)$$

Notice that our degrees of belief in the possible collections of outcomes C_k must cohere with our degrees of belief in the models M_i:

$$P(C_k) = \sum_i P(M_i) \times P(C_k \mid M_i) \qquad (A)$$

We can show that, provided this condition is satisfied, the arguments (*i*) and (*ii*) both lead to the same predictive distribution over a future outcome.

In the more realistic 'non-machine' situation, it appears that argument (*i*) cannot be applied since it is no longer meaningful to assign degrees of belief to the models. However, we might try and overcome this difficulty by assigning 'probabilities' to the M_i in such a way that the implied distribution (A) over the collections of outcomes C_k seems a reasonable representation of our degrees of belief.

We then apply the Bayesian argument, in the form (*i*), to update the 'probabilities' on the models, and hence to obtain a predictive distribution over any future outcome. In doing so, we are, of course, behaving as if the data were produced by a machine generating data in accordance with one of the models M_i. The justification for the above argument lies in its equivalence to argument (*ii*) which *is* capable of interpretation and justification in the non-machine situation.

8 CRITICISM OF THE ALTERNATIVE INTERPRETATION

In section 6, we considered a type of situation in which the Bayesian argument appears to be able to make inferences based on models that we have not explicitly considered, but that are nevertheless more appropriate than any of the models in our original set. Under our present interpretation, there is no objection to finding, after a long run of data, that the predictive distribution is represented by a mixture of the original models. So we are quite free to use the Bayesian argument to help us 'home-in' on a suitable *mixture* of models.

We have gained the freedom to use these 'mixture' arguments by dropping the requirement that the probabilities we assign to the models should be capable of direct interpretation as degrees of belief. Unfortunately, the *indirect* interpretation of these probabilities makes it very difficult to assign a prior distribution over the models.

As we have already noted, we have to fix the prior probabilities by looking at the implied degrees of belief in the collections of outcomes C_k. But surely one of our main reasons for introducing the models M_i is that it is hopelessly difficult to specify a prior probability distribution directly over the C_k—if there are n foreseeable cases, the assignment of a distribution over the C_k involves us in trying to specify an n-dimensional distribution. It is true that the models will impose some restrictions on our degrees of belief, and so reduce the number of possible distributions that we could assign but we still have to consider a wide range of implied distributions.

The difficulty of assigning probabilities to the models in such a way that they represent our prior knowledge, may well force us to assign a fairly arbitrary distribution and then wait until we have enough data to swamp it. There are two difficulties with doing this:

(*i*) We may be dealing with a situation in which we cannot obtain enough data to swamp the prior distribution.

(*ii*) Even if we have plenty of data, the initial choice of a set of models involves us in thinking about the adequacy of the set of 'mixture' models that could be generated from a given set of basic models. In order to understand the restrictions imposed on our degrees of belief by the use of a given set of 'mixture' models, we need to think about the implied distributions over the C_k. So we are back to our original problem.

CONCLUSION

We have tried to formulate the Bayesian argument in such a way that it can be logically justified. We have abandoned the idea of assigning degrees of belief directly to models and have suggested either (*i*) conditioning our models on subsets of the set of possible data sets arising from the cases in which we envisage making predictions, or (*ii*) regarding the probabilities that we assign to the models as, in themselves, uninterpretable, but nevertheless capable of giving rise to a predictive distribution that can be interpreted in a degree of belief sense.

We have seen that interpretation (*i*) fails to support current Bayesian practice—we are unable to justify either the independence assumptions made in many statistical models, or the use of 'mixture' models. Approach (*ii*) overcomes these difficulties and offers a satisfactory alternative provided that we can assign probabilities to our models in such a way that the implied distribution over the collection of possible outcomes provides a sensible representation of our degrees of belief. However, the assignment of the probabilities to the models will be more difficult than is implied by current treatments of the Bayesian argument.

Loughborough University of Technology

REFERENCES

de Finetti, B. [1975]: *Theory of Probability.*

Hacking, I. [1967]: 'Slightly More Realistic Personal Probability', *Philosophy of Science*, 34, pp. 311–25.

Harrison, P. J. and Stevens, C. F. [1976]: 'Bayesian Forecasting', *Journal of the Royal Statistical Society*, Series *B*, 38, pp. 205–47.

Hill, B. M. [1972]: 'On Coherence, Inadmissibility and Inference about many parameters in the theory of Least Squares', *Technical Report No. 25*, *Department of Statistics and Statistical Research Laboratory*, *University of Michigan.*

Leamer, E. E. [1972]: 'False Models and Post-Data Model Construction', Paper presented at fourth NBER–NSF Seminar on Bayesian Inference in Econometrics, University of Chicago.

Nelder, J. A. [1977]: 'My Kind of Statistics', *Bulletin in Applied Statistics*, 4, no. 2, pp. 77–81.

Brit. J. Phil. Sci. 29 (1978), 39–45 *Printed in Great Britain*

On the Impossibility of an Infinite Past

by G. J. WHITROW

> "The idea of infinity proves to be an auxiliary introduced by thought to facilitate its operations . . . its application to the *real world* is a misuse." (Hans Vaihinger [1924], p. 62.)

1 *History of the Problem.*
2 *Disproof of the Possibility of an Infinite Sequence of Discrete Past Events.*
3 *Unrelated Sequences of Past Events.*
4 *Transformations of Time-Scale.*
5 *Conclusion.*

The problem of the age of the universe and whether, in principle, it must be finite or could be infinite has a long history and views have been divided. It is my contention, contrary to the opinion now widely held, that the basic philosophical problem has not been resolved by Cantor's theory of infinite sets.

1 HISTORY OF THE PROBLEM

My point of view is a modern version of one that can be traced back at least as far as the Christian philosopher Joannes Philoponos of Alexandria, in the first half of the sixth century A.D. In a treatise criticising the neo-Platonist Proclos, believed to have been written not long after 529, Philoponos argued that if the world were eternal it would follow that an infinity of successive acts could have been enumerated, but this is impossible.[1] This philosophical objection to the possibility of an infinite past, *i.e.* an infinite chain of past events, has not, however, been accepted by all Christian believers in the creation of the world. A contrary opinion was expressed by St Thomas Aquinas (1225–1274), who, influenced by Aristotle, maintained, in his *Summa Theologica* (1a, xlvi, 2) 'That the world has not always existed is to be held by faith alone, and cannot be demonstratively proved.' In his opusculum *De Aeternitate Mundi contra Murmurantes*, in which he

---

[1] See Philoponos, J.: *Contra Proclum de Aeternitate Mundi*, (*ed.*) H. Rabe. Leipzig: Teubner, 1899, pp. 8 *et seq.*

Received 1 *June* 1977

criticised those whom he designated as "murmurers against Aristotle", he argued at some length that it was logically possible for the universe to have been created by God out of nothing and yet to have existed from all eternity as Aristotle believed. The medieval followers of St Augustine, himself a neo-Platonist, rejected this view. Prominent among those who argued against it was St Thomas's contemporary St Bonaventure (1221–74). Of the various arguments that he formulated against the idea of the eternity of the world[1] the most compelling is that the infinite cannot be bridged: if the universe had no beginning an infinite number of celestial revolutions must have taken place, and therefore the present day could not have been reached (Gilson [1938], p. 192). However, he was not aware of all the subtleties of infinite sets that were revealed last century by Bolzano, Cantor and others, and he was definitely mistaken when he argued that 'If no anterior day precedes the present by an infinite duration, then all the anterior days precede it by a finite duration and therefore the duration of the world had a beginning.' (*ibid.*)

The possibility of an infinite past was also rejected by the redoubtable Richard Bentley (1662–1742) in the sixth of his Boyle Sermons in 1692. He argued that the world cannot be eternal and there cannot have been an infinite number of past revolutions of a planet around the Sun. 'For, consider the *present* revolution of the Earth . . . God Almighty, if he so pleaseth, may continue this motion to perpetuity in infinite revolutions to come; because futurity is inexhaustible, and can never be spent or run out by *past and present* moments. But then, if we look backwards from this present revolution, we may apprehend the impossibility of infinite revolutions on that side; because all are *already* past, and so were once actually *present*, and consequently are finite . . . .. For surely we cannot conceive a preteriteness (if I may say so) still backwards *in infinitum*, that never was present, as we can endless futurity that never will be present. So that one is potentially infinite, yet nevertheless the other is actually finite.' (Bentley [1838], p. 134.)

The same point of view was taken by Immanuel Kant when, in discussing the first of his four antimonies of pure reason, he sought to establish the impossibility of the hypothesis that up to a given moment such as the present there has passed away an infinite sequence of successive states of the universe. 'Now', argued Kant, 'the infinity of a series consists in the fact that it can never be completed by successive synthesis. It thus follows that it is impossible for an infinite world-series to have passed away, and that a beginning of the world is therefore a necessary condition of the world's existence.' (Kant: *Critique of Pure Reason*, Kemp-Smith translation, p. 217). This argument as stated by Kant is imprecise by modern standards. Kant did not clearly distinguish between the question of whether there was, or was not, a first event in the world's history and the question of whether the total duration of past time is finite or infinite. Kant's discussion of the hypothesis that the world had no beginning in time is formulated as an argument against the idea that the past sequence of successive 'states of things' is, in the language of modern mathematics, an open set with no first member. Whether or not an infinite measure is to be associated with it depends on the choice of temporal unit.

---

[1] In particular, in his commentary on the *Sentences* of Peter Lombard, St Bonaventure (*Opera Omnia*, Quarrachi, 1882, II, 21) pointed out that, if the world were infinitely old, there would have occurred an infinite number of annual revolutions of the Sun around the ecliptic, but since in each such period there are (roughly) twelve revolutions of the Moon, *i.e.* lunar months, one infinity would thus be twelve times another—a conclusion that he rejected. Despite his rejection of this conclusion, his argument anticipates the modern idea that an infinite set, unlike a finite set, can be put in one–one correlation with a sub-set of itself.

Kant's idea of successive 'states of things' presupposes that there is a *unique* time-sequence for the whole universe. However, as I have argued elsewhere ([1966], p. 568), the objection to the possibility of an infinite past applies to any set of discrete events forming a sequence in time, *e.g.* the swings of a pendulum, or the successive clicks of a Geiger counter produced by the emission of particles from a radioactive source.

As I have already indicated, in my view Kant's argument cannot be automatically disposed of by appealing to the modern theory of infinite sets and sequences. Indeed, all reference to temporal concepts has been purged from the modern theory of infinite sets and sequences, which are thought of as aggregates and not as being produced in time. Of course, any temporal sequence of events can, if we wish, be thought of as merely an aggregate, but only by omitting all reference to its production in time. Kant's argument, however, concerns successive actions or events occurring in time. It does not conflict with the idea that infinite sequences and series in general are legitimate objects of thought, but it rejects the occurrence of an infinite sequence of past events as an actual possibility.

2 DISPROOF OF THE POSSIBILITY OF AN INFINITE SEQUENCE OF DISCRETE PAST EVENTS

Bentley's point was that an infinite sequence of past events is *not* analogous to an infinite sequence of future events. All past events are empirical certainties, in the sense that we can, at least in principle, be presented with evidence ('traces') of their occurrence. On the other hand, all future events are no more than empirical possibilities, however strongly we may happen to believe that they will in fact occur. Moreover, the reason why we tend, if we are 'rational', to place more faith, on the whole, in the occurrence of a particular past event rather than in that of a particular future event is not because retrodiction is more reliable than prediction (after all retrodiction is a form of 'prediction'), but because any genuine trace that has been left to us of the occurrence of a particular past event was made *when that event occurred*, whereas there can be no corresponding 'traces' *available now* of a particular future event made simultaneously with the occurrence of that event. That is why, on the whole, it is safer to put more trust in our memories than in our forecasts, fallible though the former often prove to be.

Let us now consider a temporal sequence of discrete events, or acts, E_n of any kind, where n takes integral values—negative, zero and positive. Since, at this stage of the argument, the question of duration need not be considered, it is unnecessary for these events to be 'evenly spaced' in time. Starting from E_0, which I designate 'the present' (*i.e.* a present event), consider the sequence of events E_n for which $n > 0$. These will be labelled 'future events'. If the set $E_n(n > 0)$ is infinite, we can describe it, following Bentley's terminology, as 'potentially infinite'. This simply means that we *assume* that each E_n will have a successor E_{n+1}, for each positive (or zero) integer n. It cannot mean that there will occur, in the fullness of time, events E_α, where α denotes the Cantorian cardinal aleph-zero. The objection to this happening is illustrated by what Bertrand Russell ([1937], p. 358) has called the 'Tristram Shandy paradox'. Tristram Shandy, in Sterne's famous novel, on finding that it took him two years to write an account of the first two days of his life lamented that material for his biography would thus accumulate faster than he could deal with it, so that he could never come to an end. 'Now I maintain', argued Russell, 'that, if he had lived forever, and not wearied of his task, then even if his life had continued as eventfully as it began, no part of his biography would have remained unwritten.' Since Tristram Shandy writes in a year the events of a day, the events of the nth day will be written about in the nth year, and since any assigned day will be the nth day, it will eventually be written about. If Tristram Shandy actually succeeded in living for an infinite number of years, then the time would eventually arrive, as Russell said, when all the days of his life would have been written about. Such a situation, however, can never in fact occur. For, as time goes on, not only does he get no nearer his goal but, on the contrary, the goal continually recedes.

Now let us consider the sequence of events E_n for which $n < 0$. If we can describe it as infinite, then each E_{-r} (writing $n = -r$) was preceded by an event E_{-r-1}. For many contemporary philosophers this appears to be all that is meant by saying that a past sequence of events is infinite. My objection to this conclusion is that since *all* past events have—and not merely *any* past event has—occurred, the situation envisaged is strictly analogous to the situation that confronts us in the Tristram Shandy paradox when we consider the possibility of events E_α eventually occurring. If n events occurred in sequence before E_0, then there must have occurred an event designated E_{-n}, in my notation. Similarly if aleph-zero events occurred before E_0, then there must actually have occurred (in time past) events $E_{-\alpha}$. But, starting from any one such event, it would have been impossible to attain the event E_0, just as from the event E_0 it will never be possible to attain events E_α in the future. Consequently, the concept of an infinite sequence of past events is incapable of culminating in the present event.[1] Hence, just as every future sequence of discrete events from E_0 onwards will always be finite, in the sense that never will an event E_n be attained where n is not finite, so in every past sequence the total number of events, however large, can never be infinite.

3 UNRELATED SEQUENCES OF PAST EVENTS

There remains the possibility that the world might be such that there are an infinite number of *unrelated* past sequences E_{-r} such that, although each sequence can necessarily only contain a finite number of events, there is no definite finite integer N such that none of the sequences concerned contains more than N members. In that sense, and in that sense only, could there be a past situation analogous to a potentially infinite sequence of future events. Nevertheless, there still could not be the possibility of an infinite number of past 'states of the universe'. For, if there is a temporal sequence of past 'states of the universe' (*cf.* the technical concept of 'cosmic time' in modern theoretical cosmology with—for the purposes of this discussion—the usual continuous variable t replaced by a sequence of discrete instants $\ldots t_{-r-1}, t_{-r}, t_{-r+1} \ldots t_0$, where t_0 denotes the present), it must itself constitute a temporal chain of the type that I have been considering, and hence the number of events in the chain, whatever this number may be, must necessarily be finite.

Hence, the only possibility that the universe does not have a finite past arises if no temporal sequence of successive states can be associated with it. Consequently, a steady-state static universe, for example an Einstein universe, is not open to objection if it is assumed that nothing ever happens in it, so that strictly speaking it is timeless. On the other hand, in the steady-state expanding world-models devised, in 1948, by Hoyle and by Bondi and Gold, respectively, although the past history of every observable galaxy is finite, there exists a unidirectional cosmic time t which is assumed to be without beginning or end. These models are therefore open to the objection discussed above, even if overall they do not change with time.[1]

---

[1] There is symmetry, in this respect, of future and past: an infinite sequence (actual infinity, or aleph-zero) of past events corresponds to an actually infinite (aleph-zero) sequence of future events. The latter concept has to be rejected and likewise, in my view, the former.

4 TRANSFORMATIONS OF TIME-SCALE

On passing from the sequence of discrete epochs . . . t_{-r-1}, t_{-r}, t_{-r+1}, . . . t_0, . . . t_{n-1}, t_n, t_{n+1}, . . . to a continuous time variable t, we can consider, as Milne and I did forty years ago in connection with the world-models based on the technique of kinematic relativity, different monotonically increasing mathematical transformations of t. For example, if $t > 0$ in a world-model that expands uniformly from an initial point-like singularity (*e.g.* the initial instant $t = 0$ corresponding to an epoch of the order of 10^{10} years ago), we can introduce another variable τ such that

$$\tau = t_0 \log (t/t_0)+t_0,$$

where t_0 is the 'present' value of t, and $\tau = t_0$, $d\tau = dt$, at $t = t_0$. Although formally τ is negatively infinite when $t = 0$, the τ-scale applies only to events for which $t > 0$ and not to the initial point-like singularity, since according to the τ-scale the model is static. If the first event after the initial singularity occurs when, say, $t = 10^{-24}$ seconds, it will be assigned a finite epoch on the τ-scale and be the first event to which that scale can be applied.

5 CONCLUSION

Kant assumed that, if the concept of time could be associated with the universe as a whole, it would be quasi-Newtonian, but he argued that the universe could not have an infinite past nor have existed for only a finite part of an infinite past (other 'leg' of the First Antimony of Pure Reason). He therefore concluded that time does not pertain to the universe itself but merely to our way of regarding it. Instead, I conclude that: *either* the universe is such that there is no 'cosmic time' (in the modern technical sense) applicable to it, *or else*—as I prefer to believe—the universe is such that cosmic time exists but its total past range is finite, in the sense explained above.

[1] These models have now been rejected by most astronomers because they are believed to be incompatible with observations, *e.g.* the cosmic background 3K black-body radiation discovered in 1965 by Penzias and Wilson.

According to current views,[1] theoretical world-models (*i.e.* mathematical models of the large-scale basic framework of the universe) ***based on general relativity*** must contain singularities, *e.g.* an initial singularity corresponding to an epoch of 'world-creation'[2] at a finite past epoch. This conclusion is consistent with current interpretations of the observational results (optical and radio) that definitely rule out, ***inter alia***, all steady-state world-models, both those without and those with mutual recessional motion of the galaxies.

Imperial College of Science and Technology

REFERENCES

BENTLEY, R. [1838]: *Sermons Preached at Boyle's Lecture, etc.* (edited by A. Dyce.) London.

GILSON, E. [1938]: *The Philosophy of St. Bonaventure* (translated by I. Trethowan and F. J. Sheed). London: Sheed and Ward.

HAWKING, S. W. and ELLIS, G. F. R. [1973]: *The Large Scale Structure of Space-Time.* Cambridge University Press.

RUSSELL, B. A. W. [1937]: *Principles of Mathematics.* Cambridge University Press.

VAIHINGER, H. [1924]: *The Philosophy of 'As If'* (translated by C. K. Ogden). London: Routledge and Kegan Paul.

WHITROW, G. J. [1966]: 'Time and the Universe' in J. T. Fraser (*ed.*): *The Voices of Time.* New York: Braziller.

[1] See Hawking and Ellis [1973].

[2] 'World-creation' signifies a finite past. To claim more than that takes us beyond science into theology.

Brit. J. Phil. Sci. 29 (1978), 47–60 *Printed in Great Britain*

Discussions

ON THE POSSIBILITY OF AN INFINITE PAST: A REPLY TO WHITROW

1 *Kant's Thesis.*
2 *Clarification.*
3 *Criticism.*
4 *Summary.*

1 KANT'S THESIS

In the thesis of the (first) antinomy of pure reason Kant argues that the world must have a beginning in time; for if one assumes that there is no such beginning, then an eternity—an actual infinity of instants—must have elapsed. But an infinity cannot be actual; for the infinity of a series consists in the fact that it *cannot be completed.* Thus the assumption that the world has no beginning is absurd; which proves the thesis that the world must have a beginning in time.[1]

2 CLARIFICATION

(1) Obviously, Kant's argument establishes (if it establishes anything) the thesis that *time must have a beginning*, rather than that the world must have a beginning in time. (This remark is quite important in the Kantian context, because Kant's antithesis is definitely not antithetical to the so corrected thesis.)

(2) Obviously, Kant's argument rests on the assumption that there cannot *exist*—in some as yet not explained sense of "exist"—a "completed" or "actual" infinity; an assumption he shares with Aristotle.

(3) The sense of "*exist*" mentioned under (2) is somehow linked with the idea of "elapsed" time, as opposed to the not yet elapsed future which, in the sense intended, does not yet "exist".

(4) The future, which is open, is, in the sense of Kant's thesis, potentially infinite: none of its instances is the last one. Thus Kant implies that there is an asymmetry with respect to infinity between the past and the future: according to Kant, *the past cannot be regarded as potentially infinite; if infinite, then it must be actually infinite. But the future can be potentially infinite.*

3 CRITICISM

The idea of the *potentially infinite* can be explained as that of the infinite sequence of natural numbers: there is no greatest natural number; but every number, however great, has a successor.

---

[1] Cp. my *Conjectures and Refutations*, p. 178.

The idea of the *actually infinite* can also be explained with the help of this same sequence. It needs the Cantorian concept of a *set*. Let us regard instead of the *sequence* of natural numbers the *set* of all natural numbers. This set is infinite, and it can be described as representing the (smallest) actual infinity.

I now state my assertion against Kant.

We can regard the past time or elapsed time and the future time or impending time as symmetrical with respect to infinity. Both may be regarded as consisting of infinite sequences of temporal units, and therefore as potential infinities; and both may be regarded as infinite sets of temporal units, and therefore as actual infinities.

(The opposite impression is due to viewing the past time (*a*) as infinite and at the same time as (*b*) having a beginning, although one that is infinitely far away in the past. But it is clear that (*b*) contradicts (*a*) and is therefore inadmissible.)

We have only to realise that we must count our units of time (say, years) from some given instant of time (say, the birth of Christ or the year A.D. 800). From that given instant we can count units (years) either in the positive direction (that of the "arrow of time") or in the negative direction. In each case we obtain a sequence of units which corresponds exactly to the sequence of natural numbers: a "potential" infinity. If, however, we consider "the whole past" or "all the units of time of the past" that is to say, the *set* of the units of time of the past, then we obtain a Cantorian actual infinity. This is precisely the same whether we look at the whole or "elapsed" past or at the whole or "not yet elapsed" future.

The difference between past time and future time is important; but it does not correspond to the difference between actual or potential infinity.

4 SUMMARY

The attempt to show by *a priori* reasoning the impossibility of time without beginning seems to me doomed to failure. There is, of course, one argument, originally due to Milne, which allows us to transform a time co-ordinate with a finite beginning into a time co-ordinate without a beginning, and *vice versa*. And in the absence of any "natural" unit of time, and even more of any guarantee that in the distant past there existed events comparable to those events we now use to define units of time, we may well wonder whether there is an ontological difference corresponding to the difference between a time co-ordinate reaching into an infinite past and a time co-ordinate with a beginning.

KARL POPPER
Penn, Buckinghamshire

EINSTEIN'S DEBT TO LORENTZ: A REPLY TO FEYERABEND AND MILLER*

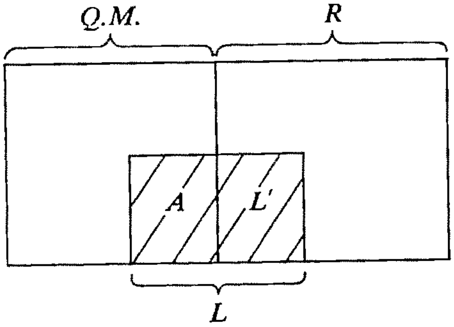
Various aspects of my [1973] paper on Lorentz and Einstein have been criticised by Feyerabend, Miller and Schaffner. In a recently published paper ([1977]) I dealt indirectly with Schaffner's interesting criticisms.[1] In the present discussion note I shall reply to Feyerabend's[2] and Miller's[3] comments. This reply will give me the opportunity to propose and defend certain theses which I did not include in my 1973 articles. For example: in answering Miller I shall be naturally led to discuss Lorentz's Theory of Corresponding States and hence the extent of Einstein's indebtedness to Lorentz.

Feyerabend maintains that

> (*i*) the transition from a research programme P to a newer and better programme Q in the same domain almost always involves a loss of content which (*ii*) is not noticed as scientists and historians use a $P' \subset P$ which has indeed much smaller content than P. P' does not occur in history, it is a logical construct, suggested by Q . . . and by the belief in increase of content.[4]

I go along with Feyerabend in accepting that there have been cases where the transition from one programme to the next involved a loss of content.[5] I doubt however that this 'almost always' occurs and propose to show that the transition from Lorentz to Einstein was precisely one of those cases in which the supersession of one programme by another did take place.

According to Feyerabend, Lorentz worked on an ambitious programme L which contained a classical atomic theory A. Let $L' = L - A$. Feyerabend maintains that the relativity programme R may have superseded L' but it certainly did not supersede the whole of L. Lorentz's atomic theory A was superseded only by Quantum Mechanics ($Q.M.$). Thus, according to Feyerabend, we have the following situation in which no two competing programmes are comparable:



---

*This paper is an expanded version of a talk given at the 1974 Nafplion Conference. I gratefully acknowledge the valuable criticisms and suggestions received from Paul Feyerabend, Adolf Grünbaum, John Stachel, John Watkins and John Worrall.

[1] Schaffner [1974].

[2] Feyerabend [1974].

[3] Miller [1974].

[4] Feyerabend [1974], p. 27.

[5] A good example given by Feyerabend himself is the transition from Aristotelian to Copernican science which involved the loss of Aristotle's realistic theory of perception, a theory which was highly confirmed.

It is my thesis that the switch from the Ether programme to Relativity is mainly a switch of heuristics. Feyerabend and I agree that the Ether had become—at least temporarily—heuristically barren. Instead of deriving the properties of the field from those of the ether, almost everything that one knew about the ether was obtained from Maxwell's equations. Like Lorentz, Einstein accepted Maxwell's equations, at least as a description of macroscopic phenomena. Moreover, *R* had the advantage of possessing a very powerful heuristic tool, namely the Covariance Principle. Finally, it ought to be mentioned that Special Relativity does not *exclude* the Ether, it simply gives us licence to choose any inertial frame as the ether frame. *R* had therefore *more* machinery at its disposal than its rival. Now it so happens that atomism was part of Lorentz's programme but not of Einstein's. Let me recall that, according to Feyerabend, atomism makes the difference between Lorentz's real programme *L* and the emasculated logical construct *L′* which I supposedly attribute to Lorentz. Let us assume Feyerabend's account is correct. But, atomism as such is neither suggested nor barred by the heuristics either of *L′* or of *R*. Like Lorentz, Einstein could have assumed—without fear of contradiction or incoherence—that the electron is a spherical distribution of electric change in space-time. As it was, Einstein in 1905 refrained from any such assumption, *i.e.* from any theory of elementary particles. I do not know the exact reasons for Einstein's agnosticism; but it is known that, because of Planck's quantum hypothesis and hence *independently of Relativity*, he had enough reservations about the *fundamental* character of Maxwell's equations not to want to base any *atomic* theory on them.[1] To summarise: had Einstein or any other relativist felt inclined to adopt atomism, he could easily have done so; he would moreover have been guided by the heuristics of *R* in constructing Lorentz-covariant laws governing the behaviour of elementary particles. In this area the relativist would have met with at least as much success—and probably also with as many difficulties—as did Lorentz. Referring to the diagram above, this means that Einstein could easily have annexed *A* to *R* but he *chose* not to do so. Thus, on the atomic front, there was stalemate between Relativity and the ether. It is not the case, as Feyerabend claims, that Lorentz's programme possessed excess content which was superseded only by *Q.M.*; both Lorentz's and Einstein's programmes were beaten in this area by *Q.M.*

This brings me to a difficulty which I did not properly tackle in my [1973], namely: given that *R* had more to offer than its rival and given that Lorentz was an eminently rational man, why was he not quickly converted to Relativity (he was of course finally converted around 1914)? In my opinion, it is because Lorentz thought he had good empirical reasons for *not* accepting Maxwell's equations as fully covariant. In his [1909] he mentioned Kaufmann's results as constituting serious evidence against the Relativity Principle. In the same book he reproduced his 1904 results without taking account of the corrections which Poincaré published in his [1905]. These corrections would have made Lorentz's electrodynamics Lorentz-covariant and hence indistinguishable from Einstein's. As soon as Lorentz found good reasons—such as the result of Bucherer's experiment[2]—for accepting the covariance of Maxwell's equations,

[1] See Einstein [1949], p. 44.

[2] Lorentz [1909], p. 339.

he realised that the ether had lost all heuristic value and consequently joined the Relativistic camp. This conversion to Einstein's approach is clearly expressed in his [1914] paper on gravitation and in the footnotes of the 1915 edition of the *Theory of Electrons*. Thus Lorentz's conversion took place before the great *empirical* successes of General Relativity, *e.g.* before the explanation of the precession of Mercury's perihelion; this, clearly indicates that heuristic considerations played an important part in Lorentz's thinking at the time.

We know that Kaufmann's results, which seemed to contradict Special Relativity, were defused by Planck. Why Einstein immediately accepted Planck's arguments while Lorentz hesitated may be due to external factors.[1] Lorentz was older and probably more conservative in his outlook than Einstein; he may have felt that the ether had rendered so many services in the past that it should not be too light-heartedly disposed of. This may have led Lorentz to take Kaufmann's results at face value; or he may simply have omitted to read Planck's account which was anyway involved and difficult to follow. In the methodology of scientific research programmes (henceforth referred to as MSRP) there is room for such external considerations. In this respect, the difference between Feyerabend and Lakatos is one of—important—emphasis. Feyerabend throws external factors into relief and ascribes great importance to them. Lakatos shows that the role of such external factors has been greatly overestimated. External factors may have caused Lorentz and Einstein to differ in their initial assessment of Kaufmann's results; but this made little difference to the final outcome, namely that Lorentz went over to the Relativity Programme.

Let me now turn to Miller's criticisms. According to Miller, 'it is quite ahistorical to consider Maxwell's equations but not the ether as part of the hard core [of Lorentz's programme]'[2] Miller overlooks the fact that I placed the ether in what I regard as the most essential characteristic of a research programme, namely its positive heuristic. There is also a good reason for not considering the ether as part of the ether programme's hard core. The hard core is by definition unalterable and there were as many different ethers as there were ether-theorists; probably even more since ether-theorists often changed their minds about the properties of their own ether. In his [1909] for example, Lorentz did not decide whether the ether was at rest or in constant motion. Had I decided to place the ether in the hard core of *L*, I would have had to restrict myself to a very weak existential statement, namely that the electromagnetic field is carried by a quasi-substantial medium called the 'ether'. In fact I did state in my [1973] that the heuristic of Lorentz's programme arises from the overall metaphysical principle that all physical phenomena are governed by actions transmitted by the ether. I could quite easily have put this—weak—metaphysical principle in the hard core of the ether programme. This would in no way have affected the theses put forward in my [1973].

Miller further maintains (p. 32) that my 'characterisation of the foundations of Lorentz's theory is incorrect' because 'Lorentz in his [1895] deduced that Newton's third law was inconsistent with the fundamental equations of electromagnetic theory'. My answer to this criticism is two fold. First, it is not at all clear that Lorentz's theory (or rather: theories) contradicts Newton's third law.

---

[1] See Zahar [1978] (forthcoming).

[2] Miller [1974], p. 32.

Poincaré pointed out that a contradiction arises *if* one abstracts from the ether and restricts oneself to what is 'observable', namely to the electrons which constitute ordinary matter.[1] Poincaré wrote:

> . . . there can be no equality between the action and the reaction, at least if we do not consider the ether but only the electrons which are alone observable, since our matter is composed of electrons.[2]

But, of course, an ether-theorist is under no obligation not to consider the ether; and let us not forget that Miller himself insists on regarding the ether hypothesis as part of the hard core of Lorentz's programme. Secondly, even if Miller's objection that Lorentz's theory contradicts Newton's third law were well-founded, it would not affect any of the theses that I put forward. Newton's third law played no part in the development of Lorentz's programme and can therefore be safely removed from its hard core. This was clearly recognized by Lorentz himself who wrote in his [1895]:

> It would be simplest to assume that no force ever acts on any volume-element of ether considered as a whole, or even that the concept of force is not applicable to such an element which anyway never moves. This conception would of course contradict the principle of the equality of action and reaction—since we have grounds for saying that the ether exerts forces on ponderable matter; but nothing, as far as I can see, compels us to elevate this principle to the rank of a fundamental law.[3]

So far. I have dealt with that part of Miller's criticism which, although partially correct, is irrelevant to my theses. I shall now try to show that his more substantial criticism is completely unfounded.

A large part of Miller's account is based on a confusion between descriptive propositions and normative criteria. That, as Miller claims, Lorentz was '*puzzled*' by Michelson's result, that he felt *dissatisfied* with his own Contraction Hypothesis (henceforth referred to as the *L.F.C.*), that he resorted to a physics of '*desperation*', these are psychological facts which may be interesting in themselves but are totally irrelevant to any objective assessment of Lorentz's achievement. Lorentz probably hoped to find a closed algebraic expression for molecular forces from which it could be deduced that such forces transform like the Lorentz-force. His failure in this direction accounts for his modesty in assessing the Molecular Forces Hypothesis (henceforth referred to as the *M.F.H.*). But Lorentz's diffidence does not make the *M.F.H. ad hoc.* He was simply wrong in conceding that 'since the nature of the molecular forces is entirely unknown to us, it is impossible to test the hypothesis.' (We know that the *M.F.H.* implies the *L.F.C.*, and Grünbaum has shown that the *L.F.C.* is independently testable[4]). A scientist's explicitly professed methodology—as distinct from his singular and often tacit value-judgments—has by itself no methodological significance. If a scientist claims that he has validly deduced his laws from the facts, what a historian has to accept is the fact that the scientist made such a claim and not that the claim is valid.

---

[1] Poincaré [1902], p. 175.

[2] Poincaré [1908], p. 222.

[3] Lorentz [1895], p. 28. In his [1909] Lorentz changed his mind about the ether being necessarily at rest.

[4] See Grünbaum [1959] and [1963].

Miller asserts that Holton's claim about the *ad hoc* character of the *L.F.C.* 'is a restatement of previously reported opinion concerning the *L.F.C.*, *e.g.* that of Poincaré. It is a historical fact which cannot be made to vanish simply by redefining the notion of *ad hoc*.' This quotation illustrates the way in which Miller conflates facts with norms. It is certainly not my intention to make historical facts vanish. But let us be clear that the putative fact in question is that, *in Poincaré's opinion*, the *L.F.C.* is *ad hoc*. This in no way implies that the *L.F.C.* must be *ad hoc* according to every normative criterion of *ad hoc*ness. At this point it is appropriate to repeat my definition of *ad hoc*ness of a theory T with respect to an observation statement e. This is all the more appropriate since Grünbaum correctly pointed out that my definition of *ad hoc*ness conflicts with a claim which I made in my ([1973], p. 101), the claim namely that *ad hoc*ness is a relation between two theories. It now seems to me that my claim was mistaken. I shall therefore drop it and retain my definition which can be spelt out as follows. Empirical non-*ad hoc*ness is a three place relation $N(H,T,e)$ between a heuristic H, a theory T and an observation statement e. This relation obtains iff T explains e and T was constructed by means of H, where H is independent of e. Thus if e is 'empirically verified', then e supports not T alone, but T and H jointly.[1] It is conceivable that two research programmes P_1 and P_2 possessing two different heuristics H_1 and H_2 should give rise to the same theory T and that a fact e should provide support for (H_1, T) but not for (H_2, T). Thus it is more appropriate to speak of support for a research programme P than for an isolated hypothesis T. This is so because P possesses a heuristic H which governs the construction of T.

Miller maintains that 'these redefinitions of "*ad hoc*" and "novelty" are contrived merely to support Zahar's contention that the *L.F.C.* is not *ad hoc* and therefore that the shift from T_1 to T_2 fits into the mould defined by the methodology of research programmes'. Miller overlooks the fact that, according to the same definition of *ad hoc*ness, Einstein's explanations of Michelson's results and of Mercury's perihelion, Copernicus's explanation of stations and retrogressions, and Bohr's explanation of the Balmer series are all non *ad hoc*. It is therefore hardly the case, as Miller claims, that my 'notion of *ad hoc* is itself *ad hoc*'.[2]

In an attempt to rescue Holton's notion of *ad hoc*ness Miller puts forward a definition which leads to paradoxical results. Miller proposes that '*ad hoc* with respect to x means not derivable from x'. From this it follows that 'non *ad hoc* with respect to x' is equivalent to 'derivable from x'.[3] Thus, only if the whole of Relativity Theory were derivable from Michelson's result, would Relativity be a non *ad hoc* explanation of Michelson's experiment. More generally, if I simply describe the outcome of any experiment, I would be giving a non *ad hoc* explanation of that experimental result. I of course maintain that no scientific theory, because of its universal character, is deducible from an experimental result; in which case every theory will, according to Miller, be *ad hoc* with respect to every experimental result. Both consequences of Miller's definition are clearly absurd.

---

[1] I realise that this concept of support is intuitive and that it is open to objections, because H is not a proposition but a set of directives. However, I can see no way round this difficulty. For a clearer and more general exposition of the notion of empirical non-*adhoc*ness, see Worrall [1978] (forthcoming).

[2] Miller [1974], pp. 30–1.

[3] This paradoxical consequence of Miller's definition was first pointed out to me by A. Grünbaum.

Let me now turn to another criticism which underlines the problems posed by the role of rational reconstructions in the history of science.[1] Miller writes ([1974], p. 30):

> Zahar's argument for the non *ad hoc*ness of the *L.F.C.* turns on two points: one which concerns methodology; the other involves a convenient distortion of historical facts which enables Zahar to fit Lorentz's work of 1892–1904 into the structure of the methodology of research programmes.[1]

The general question of whether rational reconstructions are necessary, dispensable or undesirable has given rise to much controversy in recent times. I shall therefore start by dealing very briefly with this general question from the viewpoint of MSRP.

It is a platitude that reconstructions are necessary because the historian does not know all the facts. There is in particular one type of fact to which the historian of science can in principle have no *direct* access; namely the intuitive system of appraisal which the scientist applies either in constructing his own theories or in choosing between several existing programmes. In certain cases scientists have written about their methodology and their heuristics. But we have seen that the scientist's reports cannot always be taken at face value: he may either knowingly disguise the truth, or he may be sincere but have a false consciousness of his own activity. This is why one ought to follow Einstein's advice that, in order to assess a scientist's work, one has to concentrate on what he does and not on what he says. Even the historian who decides to accept the scientist's account of his own heuristics is already reconstructing history; such a historian would in effect be subscribing to the very bold hypothesis that the scientist *knows* and *says* the truth about himself.

Reconstructions are therefore not a luxury but a sheer necessity. Of course, a necessary condition for a reconstruction to be allowable is that it be consistent with the accepted facts. The historian who attributes to a scientist a given methodology has to do so in a manner compatible with what is known about the scientist's discoveries and publications. Consistency with the facts may appear to be a stringent condition, but it is in effect very weak. Consider for example a historian who puts forward a methodology *M* to which a scientist *S* is supposed to adhere *intuitively* (*i.e.*, in the majority of cases, *S* will be incapable of articulating *M* in general terms). Suppose it is found that in some situations in the past *S* violated *M*. The historian can always adduce certain external factors—psychological, sociological or even physiological factors—which 'explain' why *S*'s actions did not conform to *M*. It is precisely because consistency with the facts is a weak constraint that the notion of a *rational* reconstruction becomes all-important. *An allowable reconstruction is said to be rational if it minimises the recourse to external factors for which there exists no independent evidence.* Obviously no reconstruction is entirely rational, but one reconstruction can be more rational than another. For example, it is an accepted fact that Einstein read Lorentz's [1895] where the Michelson experiment is discussed at length. It is also a fact that Einstein denied that Michelson's results played any inspirational role in the genesis of Special Relativity. A historian according to whom Einstein was an inductivist could say that Einstein was lying or that he had simply forgotten about the origins of Special Relativity. Holton[1] convincingly showed that, throughout his life, Einstein, though an admirer of Michelson, always stopped short of saying that the discovery of Special Relativity had anything to do with Michelson's experiment. Thus, the inductivist is left with the hypothesis that Einstein systematically lied throughout his life about the role of Michelson's experiment. This is a logically possible explanation but it is out of character with Einstein who was known for his intellectual honesty and who, in other circumstances, freely admitted his debt to other scientists. So the inductivist account is consistent with the facts but there exists no independent evidence for the psychological hypothesis that Einstein, although an honest man, always lied in connection with Michelson's result. There is of course another solution open to the inductivist historian; he could accept that Einstein was telling the truth and assume that some external factor caused Einstein to act irrationally, *i.e.* to violate the canons of inductivism by basing Special Relativity on speculation. But, while the inductivist historian would have to specify these external factors and find some independent evidence for their existence, the historian who subscribes to MSRP needs resort to no external considerations at all. In the absence of independent evidence an inductivist reconstruction is less rational than one made in the light of MSRP.

---

[1] My account will differentiate between reconstructions of history (which I shall claim to be necessary) and *rational* reconstructions (which, though they may be desirable, are not necessary). Lakatos tends to conflate these two notions in his [1971]. For a further clarification of the notion of rational reconstructions see section 5 of Worrall [1976].

Now let me go back to Miller's criticism of my views. I propose to show that Miller's own reconstruction of the discovery of *L.F.C.*, far from being rational, is even inconsistent with the accepted historical facts. According to Miller 'it is clear from Lorentz's [1892*b*] and [1895] that his methodology was to posit the *L.F.C.* specifically to account for the Michelson and Morley experiment. Moreover he obtained the *L.F.C.* from an argument based upon the Newtonian addition law of velocities. *Then*, Lorentz posited the *M.F.H.* for the purpose of constructing a plausibility argument in support of the *L.F.C.* that involved the use of certain concepts from his electromagnetic theory . . . !' Also: 'if one wishes to place this calculation within a hypothetico-deductive scheme then one is forced to say that Lorentz deduced the *L.F.C.* from a law of mechanics'.[2] Finally, in Miller's opinion 'Zahar asserts that "Lorentz deduced the *L.F.C.* from a deeper theory", namely from what he calls the *M.F.H.* However, as we have just seen, Lorentz did no such thing. Moreover, the *M.F.H.* does *not*, contrary to Zahar, constitute a theory about Molecular Forces' (p. 39).

I can make neither head nor tail of the last sentence in this last quotation. The *M.F.H.* is the proposition that molecular forces are affected by every translation through the ether in the same way as the Lorentz-force. Thus the *M.F.H.* is trivially a proposition *about* molecular forces. Turning to Miller's other claims, it is obvious that, if they were true, then he would be automatically landed with two difficulties.[3] First: we know that Lorentz had been, as he put it, 'puzzled' by Michelson's experiment since 1887. How is it that it took him five long years to arrive at a hypothesis which is deducible from Michelson's result together with a simple law of mechanics? Secondly: why did it take Lorentz exactly the time necessary for the discovery of the *L.F.C.* to follow immediately upon the discovery of his famous transformation? We know that there exists a logical connection between the Lorentz transformation and the Contraction Hypothesis. Are we to believe that Lorentz was unaware of this connection and that some 'cunning of reason' made him produce his results in the correct logical order? We are told by Miller that Lorentz was looking for a plausibility argument and that he somehow found it in 1892. But why *in 1892*? Moreover, Miller does not explain the nature of this plausibility argument. He simply quotes a passage from Lorentz's [1892*b*] in which the author says it is not inconceivable nor even far-fetched to suppose that the dimensions of a body are affected by its motion in the ether. Miller omits to quote the next few paragraphs of Lorentz's [1892*b*] which contain not a plausibility argument but *a proof* of the *L.F.C.* from the *M.F.H.* I shall therefore return Miller's compliment and maintain that such an omission amounts to a distortion of history in the light of preconceived value judgments. The missing paragraphs are as follows:

---

[1] Holton [1969].

[2] Miller [1974], p. 35.

[3] Later I shall argue that Miller's claims are in fact false.

> Let A be a system of material points carrying certain electric charges at rest with respect to the ether; B the system of the same points while moving in the direction of the x-axis with the common velocity p through the ether. From the equations developed by me [Lorentz is referring to his [1892*a*]], one can deduce which forces the particles in system B exert on one another. The simplest way to do this is to introduce still a third system C, which just as A, is at rest but differs from the latter as regards the location of the points. System C namely can be obtained from system A by a simple extension by which all dimensions in the direction of the x-axes are multiplied by the factor $(1+p^2/2V^2)$ and all dimensions perpendicular to it remain unaltered.
>
> Now the connection between the forces in B and C amounts to this, that the x-components in C are equal to those in B, whereas the components at right angles to the x-axis are $(1+p^2/2V^2)$ times larger than B.
>
> We will apply this to molecular forces. Let us imagine a solid body to be a system of material points kept in equilibrium by their mutual attractions and repulsions and let system B represent such a body whilst moving through the ether. The forces acting on any of the material points of B must in that case neutralise. From the above it follows that the same cannot then be the case for system A whereas for C it can . . .[1]

Thus, contrary to Miller's claim, Lorentz established that (using the standard symbols for velocities), if the molecular and the electromagnetic forces obey the same transformation law, all bodies will be contracted by the factor $1-\frac{v^2}{2c^2} \doteqdot \left(1-\frac{v^2}{c^2}\right)^{\frac{1}{2}}$ along their direction of motion. (Note that in determining the way in which the Lorentz force is affected by motion through the ether, the Lorentz transformation plays an essential role.) In other words Lorentz showed that in predicting the (incorrect) outcome of Michelson's experiment, physicists had made the following tacit assumption: the molecular forces behave so differently from the electromagnetic ones that the former exactly compensate for the effects of the latter and thus keep the dimensions of a moving body constant. From the viewpoint of the ether programme, according to which all forces are states of the same substratum, this hidden assumption is a very *implausible* conspiracy theory. Every ether theorist like Lorentz, who was anyway inclined to reduce everything to electromagnetism, would naturally adopt the opposite assumption, namely that *all* forces transform alike. Note that this heuristic line of reasoning, which leads to the *M.F.H.*, is totally independent of the *actual* outcome of Michelson's experiment. Hence the non *ad hoc* character of the *M.F.H.*

---

[1] Lorentz [1892*b*], p. 222.

It is of course true that there is both in Lorentz's [1892*b*] and in his [1895] another line of reasoning which leads not to one but to a whole spectrum of *ad hoc* hypotheses. If we assume that the horizontal and transverse dimensions of a moving body are contracted by the factors $1+\delta$ and $1+\epsilon$ respectively, then Michelson's result is saved if: $\epsilon-\delta = v^2/2c^2$. Since we have one equation in two unknowns ϵ and δ, the parameter δ can be fixed arbitrarily. To each value of δ corresponds an *ad hoc* contraction hypothesis, *i.e.* a contraction hypothesis specifically cooked up in order to account for Michelson's experiment.

Miller picks on this *ad hoc* heuristic path precisely because he wants to impose an accepted value judgment on Lorentz's achievement. He is partially correct in saying that Lorentz deduced the *L.F.C.* from the Newtonian addition law of velocities; in fact Lorentz showed that he could 'deduce' an infinity of contraction hypotheses from Michelson's result. It does *not* follow from this however that Lorentz did *not* also deduce the *L.F.C.* from the *M.F.H. By the way, Lorentz was aware that his two lines of reasoning were independent of each other.* This is clearly indicated by the following quotation: 'It may be *worth mentioning* that the result obtained in the case of electric forces yields, when applied to molecular forces, *exactly the value given above for* α'.[1] Both heuristic paths were retraced in Lorentz's [1895]: the *non ad hoc* one in section 23 and the *ad hoc* one in section 90. Only section 90 was included in the 'Principle of Relativity'[2] which was later translated into English. Given the popularity of this book, it is hardly surprising that a whole myth arose about the *ad hoc* character of the Lorentz–Fitzgerald Contraction Hypothesis.

I shall finally show that Miller's views about the origins of the Lorentz transformation are untenable. This will give me the opportunity of saying something about Einstein's debt to Lorentz. Miller writes (p. 39):

> Moreover, the famous transformation that Zahar mentions . . . is not what can be considered even with hindsight, as the 'Lorentz transformation of 1895'. As I mentioned previously, there were two sets of transformations in the [1895]: equations (18)–(21), and equations (25)–(28). The former set was used in the passage from the [1895] quoted [by Zahar]. The latter set is what can, in retrospect be considered by us as (*c.* 1895) the 'Lorentz transformations'. They are the set Lorentz used to prove the theorem of corresponding states. Zahar is obviously unaware of this fact.

---

[1] Lorentz [1892*b*], p. 222. $(1-\alpha)$ is the ratio of the transverse to the horizontal dimension of a moving body. Note again the typically modest tone of Lorentz's style.

[2] See Einstein and others [1923].

The equations to which Miller refers in this passage are essentially as follows:

$$x' = \gamma x_r \; [= \gamma(x - vt)] \tag{18}$$

$$y' = y_r \; [= y] \tag{19}$$

$$z' = z_r \; [= z] \tag{20}$$

$$t' = t_r \; [= t] \tag{21}$$

where $\gamma = (1 - v^2/c^2)^{-\frac{1}{2}}$. And:

$$x_r = x - vt \tag{25}$$

$$y_r = y \tag{26}$$

$$z_r = z \tag{27}$$

$$t_r = t - \frac{v}{c^2} x \tag{28}$$

Let me immediately point out that (21) does *not* occur in sections 19–23 of the [1895] to which I refer in my [1973]. Lorentz goes from the absolute coordinates x, y, z, t to the Galilean coordinates x_r, y_r, z_r, t_r where:

$$x_r = x - vt, \qquad y_r = y, \qquad z_r = z, \qquad t_r = t$$

At this point Lorentz observes that, since he is considering a system of charges at rest in the moving frame, the field quantities will be independent of t_r. He then introduces a new transformation which affects the spatial variables only. This transformation consists of equations (18)–(20) alone. The relations (18)–(20) are the first three equations of the transformation law which was first published in the [1892*a*] and was as follows:

$$x' = \gamma x_r = \gamma(x - vt) \tag{18'}$$

$$y' = y_r = y \tag{19'}$$

$$z' = z_r = z \tag{20'}$$

$$t' = t_r - \gamma^2 \frac{v}{c^2} x_r = \gamma^2 \left(t - \frac{v}{c^2} x \right) \tag{21'}$$

We see that, except for the extra factor γ in the expression of t', equations (18′)–(21′) constitute the well-known Lorentz transformation.

Note that (21′) is very different from (21). It is my conjecture that Miller *invents* (21) *in order to try to disconnect the discovery of the L.F.C. from that of the Lorentz transformation.* But we have seen that such efforts are futile because, as early as 1892, Lorentz recognised the connection between the *M.F.H.* and the *L.F.C.* by way of the Lorentz transformation. Equations (25)–(28), which differ from (18′)–(21′) by second order quantities, do not constitute, as Miller claims, a predecessor in 1895 of the Lorentz transformation. A much more general Lorentz transformation, namely equations (18′)–(21′), had already been discovered *in 1892!*

In order to understand the role of equations (25)–(28), we have to consider Lorentz's 1904 theory, *i.e.* what I call, for want of a better word, the Theory of Corresponding States.[1] This theory has two heuristic ancestors each of which is in different ways both more restricted and more general than the other. One heuristic ancestor is the Lorentz transformation of [1892*a*], *i.e.* equations (18′)–(21′). These equations are *restricted* to the space-time coordinates, but they apply to *all* frames moving with a velocity smaller than c. The second ancestor is the theorem of corresponding states which is ***restricted*** to cases where second-order quantities can be neglected, *i.e.* where (25)–(28) can be used instead of (18′)–(21′). However this second ancestor introduces a very important *new idea*: apart from transforming the coordinates, Lorentz changed $\vec{E}$ and $\vec{H}$ in such a way that Maxwell's equations keep their form. In other words, the transformed field quantities $\vec{E}'$ and $\vec{H}'$ obey Maxwell's equations with respect to the transformed coordinates x', y', z', t'. (Of course to within second order quantities in v/c). *This is the theorem of corresponding states which expresses the first-order covariance of Maxwell's equations.* In his [1895] Lorentz used this theorem in order to show that no first-order effects arise from experiments carried out with terrestrial light sources.

---

[1] For more details *cf.* Zahar [1973], pp. 59–63.

Lorentz tried to fuse these two approaches, first unsuccessfully in 1899 and then much more successfully in 1904. In other words, he tried both to use the full-fledged Lorentz transformation and to change the field quantities in such a way that Maxwell's equations keep their form. This synthesis was achieved in 1904. (Due to a faulty interpretation of the transformed density ρ' the synthesis was not entirely successful.)

Turning to Einstein, my reconstruction of his heuristics is as follows. Einstein had read both Lorentz's [1892*a*] and his [1895]. He was therefore acquainted with the Lorentz transformation and with the theorem of corresponding states. This means that he knew about the first-order covariance of Maxwell's equations and its connection with the absence of first-order effects. Disbelief in accidents was a trait of Einstein's scientific character. For him the first-order covariance of Maxwell's equations together with the Classical Relativity Principle were only the tip of a very large iceberg. So Einstein extrapolated by assuming that all laws assume the same form in all inertial frames. In other words, Einstein put Lorentz on his feet: instead of attempting by trial and error methods to construct $\vec{E}'$ and $\vec{H}'$, Einstein started by supposing the problem already solved; *i.e.* he assumed the covariance of Maxwell's equations and then worked backwards. From the covariance of Maxwell's equations he derived the invariance of c. Since c is a velocity, its invariance must have strong kinematical repercussions. Being already familiar with the Lorentz transformation, Einstein knew roughly what kinematics to expect; he must for example have known that t transforms together with x, y, z and is therefore frame-dependent. It is very likely that this foreknowledge of the end-result helped Einstein to derive the Lorentz transformation from the invariance of c and the Relativity Principle. Having transformed the coordinates, he went back to Maxwell's equations and from their covariance determined the transformation laws for $\vec{E}$, $\vec{H}$ and ρ. Not only do we see that Einstein's indebtedness to Lorentz was very great; we also understand why Einstein's and Lorentz's theories were so similar. Both theories arose from Lorentz's [1892*a*] and [1895].

I realise all the above is nothing but a reconstruction. But since we know rather little about the origins of Special Relativity, I hope that my reconstruction, unlike Miller's, is at least consistent with the accepted facts.

ELIE ZAHAR
London School of Economics

REFERENCES

EINSTEIN, A. and others [1923]: *The Principle of Relativity.*

EINSTEIN, A. [1949]: 'Autobiographical Notes', in P. A. Schilpp (*ed.*): *Albert Einstein: Philosopher-Scientist*, pp. 1–95.

FEYERABEND, P. K. [1974]: 'Zahar on Einstein', *The British Journal for the Philosophy of Science*, **25**, pp. 25–28.

FEYERABEND, P. K. [1975]: *Against Method.*

GRÜNBAUM, A. [1959]: 'The falsifiability of the Lorentz–Fitzgerald contraction hypothesis', *The British Journal for the Philosophy of Science*, **10**, p. 48.

GRÜNBAUM, A. [1963]: 'The bearing of philosophy on the history of science', *Science*, **143**, pp. 1406–12.

HOLTON, G. [1969]: 'Einstein, Michelson and the "crucial" experiment', *Isis*, **60**, pp. 133–97.

LAKATOS, I. [1971]: 'History of science and its rational reconstructions', reprinted in C. Howson (*ed.*): *Method and Appraisal in the Physical Sciences*, 1976.

LORENTZ, H. A. [1892*a*]: 'La théorie électromagnetique de Maxwell et son application aux corps mouvants', *Arch. Neerl*, **25**, pp. 363–642; reprinted in *Collected Papers*, **2**, pp. 164–343.

LORENTZ, H. A. [1892*b*]: 'The relative motion of the earth and the ether', *Versl. Kon. Akad. Wetensch. Amsterdam*, **1**, pp. 74–8; reprinted in *Collected Papers*, **4**, pp. 219–23.

LORENTZ, H. A. [1895]: *Versuch einer Theorie der elektrischen und optischen Erscheinungen in bewegten Köpern;* reprinted in *Collected Papers*, **5**, pp. 1–138.

LORENTZ, H. A. [1909]: *The Theory of Electrons.*

LORENTZ, H. A. [1914]: 'La Gravitation', *Scientia*, **16**, 1914; reprinted in *Collected Papers*, **7**, pp. 116–46.

MILLER, A. I. [1974]: 'On Lorentz's methodology', *The British Journal for the Philosophy of Science*, **25**, pp. 29–45.

POINCARÉ, H. [1902]: *La Science et l'Hypothèse.* Page reference is to the English translation: *Science and Hypothesis.*

POINCARÉ, H. [1905]: 'Sur la dynamique de l'électron', *Comptes Rendus de l'Academie des Sciences*, **140**, pp. 1504–8.

POINCARÉ, H. [1908]: *Science et Méthode.* Page reference is to the English translation: *Science and Method.*

POPPER, K. R. [1972]: *Objective Knowledge.*

SCHAFFNER, K. [1974]: 'Einstein versus Lorentz: Research programmes and the logic of comparative theory evaluation', *The British Journal for the Philosophy of Science*, **25**, pp. 45–78.

WORRALL, J. [1976]: 'Thomas Young and the "refutation" of Newtonian optics: A case study in the interaction of philosophy of science and history of science' in C. Howson (*ed.*): *Method and Appraisal in the Physical Sciences.*

WORRALL, J. [1978]: 'The ways in which the methodology of scientific research programmes improves on Popper's methodology' (forthcoming).

ZAHAR, E. [1973]: 'Why did Einstein's programme supersede Lorentz's?' *The British Journal for the Philosophy of Science*, **28**, pp. 195–213, 223–62.

ZAHAR, E. [1977]: 'Mach, Einstein and the rise of modern science', *The British Journal for the Philosophy of Science*, **28**, pp. 195–213.

ZAHAR, E. [1978]: 'Crucial experiments: a case study' (forthcoming).

Review Articles

UNRAVELLING THE WORLD KNOT: SCIENTISTS AND PHILOSOPHERS ON THE MIND-BRAIN CONTROVERSY*

The idea behind this book was to take three psychoanalysts, three neuroscientists, three philosophers of science, a psychologist and an engineer, all interested in the mind–brain problem, bring them together for a few days on a sunny beach in California, then encourage them to write up their reflections, the edited result constituting a fresh multidisciplinary assault on Schopenhauer's 'world knot'.

Of course it is nothing of the sort. With the exception of two contributions that support a third, everyone here is writing from a previously staked-out, fairly well-known and deeply entrenched position. There is very little indication that anyone learned anything from anyone else. In particular there is a revealing asymmetry between the neuroscientists and the philosophers on this problem. The neuroscientists know a lot about the brain but differ wildly on its relation to the mind, while the philosophers know almost nothing about the brain but are to a man confident it is the mind.

The first contributions to the volume, if taken seriously, obviate all the rest. Walter B. Weimer, a psychologist, insists despite all the disclaimers of identity theorists in the literature that mind–brain dualism is a problem of *meaning*, that the difference in our mental and physical languages reflects deep structural ambiguities in the brain, and that a transformational analysis will not only do away with the problem by revealing 'the mental' and 'the physical' to be surface manifestations of the deep structure, but bring us as close as we can get to the 'pure events' constituting neutral reality. Anyone who thinks the mind–brain problem appropriately modelled by Necker cubes or ambiguous sentences can stop right there. Or, if that is not quite enough, he can go on to Peter H. Knapp's psychoanalytic 'hour' in the course of which he will learn that if we would only regress to early childhood on the analyst's couch the problem, which is just the result of bifurcating reality in order to maintain emotional security in a threatening adult world, will go away. Similarly another psychoanalyst, Irwin Savodnik, traces the aetiology of the problem to the transition from 'expressive' language functions to 'representative' ones, where we begin to impose concepts on our experience: it is 'symbolic consciousness' which is the real culprit. No doubt small children and languageless animals do not puzzle over the relation between mind and body (for that matter most linguistically competent but philosophically untainted adult humans do not), and no doubt anyone with a suitable lesion to Broca's area will stop puzzling over it, but it does not follow from the fact that one needs symbolic language to state the problem that it is such language which creates it.

---

*Review of G. G. Globus, G. Maxwell, and I. Savodnik (*eds.*) [1976]: *Consciousness and the Brain: A Scientific and Philosophical Inquiry*. New York and London: Plenum Press. $29.40. Pp. *xii*+365.

Assuming one is not impressed by these therapeutic approaches, one can then go on to the meat of the book, which really begins with John C. Eccles's discussion of the relation between known cerebral function and our subjective experience of freely willing an action. Unfortunately, Sir John's exposition is flawed by his continuing insistence that the minor or non-dominant (usually the right) hemisphere of man is unconscious, or at least not self-conscious, on grounds that it does not speak. However as Roger W. Sperry has recently revealed (Sperry [1977]), the mute hemisphere in split-brain subjects can not only recognise photographs of the patient (and of friends, relatives, public figures), but is capable of expressing manually (with a 'thumbs-down' gesture of the left hand) and facially (with a wry grin) mock disapproval of 'himself'! It is just not plausible that an entity displaying self-deprecating humour should be lacking in self-consciousness, let alone in consciousness.

Putting that aside for the moment, it is important to recognise that while John Eccles is the only protagonist in this volume to take a frankly dualistic position in the Cartesian–Sherringtonian tradition, he is also the only one to bring into the discussion evidence that has, or could have, a direct bearing on the relation between brain and mind. This evidence comes from two sources. The first source is Penfield's work in electrical stimulation of the exposed motor cortex in waking epileptic patients (Penfield and Rasmussen [1950]; Penfield and Roberts, [1959]). If, for example, the precentral gyrus of the left hemisphere is stimulated half-way down the Rolandic fissure, the subject's right index finger may suddenly flex. According to Penfield, when this happens the subject invariably disowns the action, saying something like: 'That is due to something done to me and is not done by me.' Eccles then brings in evidence from a second source, the work of Kornhuber and his associates (Deeke, *et al.* [1969]), which shows that preceding rapid *voluntary* flexion of the right index finger there is a slowly rising negative potential (recorded from the scalp), called the 'readiness potential', beginning about 800 msec in advance and covering wide areas of frontal and parietal cortices in both hemispheres; this culminates in a sharp negativity over the left precentral hand area of motor cortex (where Penfield had his stimulating electrode tip in the above example), only 50 msec prior to onset of movement of the right index finger. From these data Eccles hypothesizes that the initially bilateral widespread wave of activity consists of neuronal *responses* to the mental act of willing to move one's finger.

For this, *inter alia*, he is subjected to an eponymous attack by C. Wade Savage, who says that proving the brain is *not* influenced by a soul would be as hard a task as proving that God does not intervene in the physical universe; it is a practical impossibility, but presumably only a practical one, that every neuron in the brain cannot be simultaneously monitored to show that its activity is in fact caused by other physical events and exclusively so. Then Savage adds, rather glibly, that '... neurophysiological investigations have so far failed to uncover any neural events not caused by physical events' (p. 151).

But how would one know a neural event is *not* caused by physical events? If that neural event were, *ex hypothesis*, caused by a purely mental event, you would by definition be unable to observe the mental event which is causing it. So the only way to uncover neural events not caused by physical events would be to consistently fail, in all the right circumstances, to find physical events (*i.e.* other neural events) causing that neural event. Now let us bring the contrary positions of Eccles and Savage to bear on the data mentioned above. Eccles says that the readiness potential reflects cerebral ***responses*** to the mental act of willing to move one's finger. Savage would have to say the neuronal activity so signalled ***is*** the mental act of willing to move one's finger, *i.e.* that they are contingently identical sets of events, processes or states. He would have to say this because there is no better candidate brain process, whether he knows that or not.

But if so, then our empirical expectation would be quite clear. While stimulation of the precentral hand area in the left hemisphere causes the subject's right index finger to flex and he or she disowns the action, this is because the readiness potential did not occur, the subject not having willed the movement. By the same logic, stimulation of wide areas of frontal and parietal cortex in both hemispheres should at least sometimes ***cause*** the readiness potential to appear and ***cause*** the subject to report: 'You know, that's funny, I suddenly ***wanted*** to move my finger.' Yet so far as this writer is aware, in all the stimulation studies done by Penfield and others, the only reports of a subjective desire to move came from stimulation of the so-called 'second sensory' area on the precentral cortex just anterior to Heschl's gyrus. This is a very small area, certainly too delimited to be associated with the whole spread of the readiness potential, and too far from representation of motor movement for the index finger on the precentral motor strip. Besides, stimulation of this area actually ***inhibits*** execution of a desired movement. But in any case the example shows us that Eccles's position is in principle falsifiable, while Savage's general disclaimer of purely mental events ever causing neural events in the brain is clearly not, any more than the hypothesis that God does ***not*** intervene in the universe.

It would be useful now to try to put together, in a scientific context, both strands of Sir John's thinking in this contribution: his claim that only the speaking hemisphere is fully conscious, and his claim that the readiness potential is a cerebral response to mentally willing limb movements. At the end of his paper he notes that it would be especially interesting to investigate the distribution of that potential over the disconnected cerebral hemispheres of the split-brain patient. But then he remarks:

> It would be predicted that, for willed movements of the right hand, the readiness potential would have a distribution corresponding to its generation only in the left hemisphere [because the surgery prevents bilateral hemispheric involvement]. Necessarily the experiment cannot be done when the subject is attempting to carry out willed movements of the left hand. Since these movements do not occur, it is not possible to carry out the recording procedures, which necessitate a backward computation triggered by the initiation of the movement (p. 119).

Sir John surely does not mean that the right hemisphere cannot move the left hand. He must mean that these are not consciously ***willed*** movements, because in his view this hemisphere, being speechless, is incapable of consciously willing to move it. If that were true, then no readiness potential would be found distributed over the non-speaking hemisphere. But to say that ***necessarily the experiment cannot be done*** is only to invoke his own preconceptions about what would or would not be found. Of course the experiment can be done. And when it is, this reviewer is willing to wager that exactly the same readiness potential will be found distributed over the mute right hemisphere preceding left finger or hand movement as is found distributed over the verbal left hemisphere preceding right finger or hand movement of the same patient. If this contrary prediction proves correct, it follows in logic that Sir John would have to admit that purely mental willing of limb movements can be initiated, in the commissurotomy patient anyway, by either hemisphere. And if he wants to call the agent in each case a 'soul', it follows that the human 'split' has two souls, though one of them cannot talk.

I pass on now to Roger W. Sperry's contribution, and to the support it gets from two other contributors to the volume, E. M. Dewan and William C. Wimsatt. In the past this reviewer, like many others who have read earlier expressions of Sperry's views on mind and brain, found it difficult to understand exactly what he is maintaining (Puccetti [1977]), largely because of Sperry's tendency to override traditional terminological distinctions. However in the brief exposition under review Sperry seems to go farther than he has elsewhere to acknowledge that his 'emergent interactionist' view of consciousness is, after all, compatible with an upgraded psychoneural identity theory, provided this recognizes that holistic, configurational properties of brain circuits supervene in the control of, and are not reducible to, their neuronal components. Dewan backs this up with an example from electrical engineering. A grid of power generators will pull together in frequency; from this mutual entrainment of oscillations there emerges a 'virtual governor' of the whole system which supervenes in the behaviour of the individual units without being identifiable with any of them, or even with all of them together. (In this way can a whole be greater than the sum of its parts.) Similarly, Wimsatt interprets Sperry's 'interactionism' as explanatory rather than ontological, and as a perfectly justified denial of 'interlevel reductivism', the claim that molar physical properties admit of a reductivist explanation at molecular level. On his view, physicalist explanations of consciousness should be amended in the direction Sperry suggests, but of course this has not the slightest tendency to show physicalism false.

If these assessments of Sperry's position are correct—and it is a pity the volume does not contain his explicit reactions to them—then one can put Sperry down as being, on this issue, just a more sophisticated psychoneural identity theorist. In his own contribution he merely says that the postulated holistic, configurational or pattern properties of brain circuits that will explain consciousness have yet to be discovered (p. 166), but leaves no doubt that he thinks they will and hence that they, too, will admit of a physical description. If so, this reviewer has but one question to put to these and like-minded participants in the conference. Why could not the brains of higher animals have evolved exactly as they did, with all the molecular and presumed configurational properties their brain circuits have, without *any* of this neural machinery being contingently identical with conscious experience? One does not see how physicalism in *any* form, given its theoretical assumptions, could possibly answer such a question, though it is precisely this question which any neo-Darwinian would want an answer to.

The same problem arises in connection with the next contribution, by

Gordon G. Globus, his so-called 'complementarity solution' to the mind–brain problem, presented as a psychoneural *structural* identity thesis. Mind and brain have incompatible properties and thus are not reducible to each other, but that is because one and the same system with an identical structure has an *intrinsic* perspective of conscious awareness and an *extrinsic* perspective of neural organisation. But again, *why is there* any intrinsic perspective if matter, as extrinsic perspective, does not depend on there being one to have evolved neural systems?

However most readers' attention will be caught by the panpsychism entailed in Globus's theory. Brains and rocks, for example, differ enormously in organisational complexity, but it is only a difference in degree. So on his theory even rocks will have some intrinsic awareness, however little compared to brains. The measure of the degree of consciousness in physical systems, he says, '. . . is the richness of structure of that system per unit volume and time' (p. 290).

But in that case cerebellar cortex (and the brainstem, for that matter) is more conscious than cerebral cortex, because it contains far more neurons per unit volume (Llinás [1975]), and these are active even during sleep when, for example, we make postural adjustments. Yet as Sperry mentions (this volume, p. 324), stimulation of the cerebellum in a conscious subject yields no conscious effect. Globus's reply, that all this suggests is that the cerebellum, like the minor hemisphere of a split-brain patient, cannot report its conscious experience because it isn't hooked up to the language system, is not only facetitious but false. There are rich connections between the cerebellum and the cerebral cortex of both hemispheres, and thus, indirectly, to the language centres of the major hemisphere. If its purpose were to relay sensory information we would feel stimulation of the cerebellum far more than, say, stimulation of the ulnar nerve in the wrist. Thus the conscious awareness of the cerebellum, which on Globus's theory should be greater than that of the cerebrum, remains no more than an abstract possibility, in that respect on a par with consciousness in rocks.

Karl Pribram, whose contribution follows, makes the hard-headed clinician's observation that the only kind of consciousness which makes a difference is the kind one can lose (and sometimes permanently) in a medical emergency. However when it comes to explaining the relation between conscious experience and the brain, Pribram's ideas do not seem to deepen our understanding of this in any clear way. One fails to see, for example, the relevance of holography to the brain's processing of even optical information, let alone auditory or somaesthetic input. Those contributors who hint that an analogy between holonomic processes and conscious experience will overcome Sellar's 'grain objection' to the mind–brain identity thesis (pp. vi, 355) appear to be grasping at straws. The phenomenal field is not a random configuration of dots any more than it is a digital 'on–off' type of thing.

Grover Maxwell, in leading the (edited) 'Conversation' between philosophers and scientists which follows, and in his 'Afterthoughts' on that discussion which conclude the volume, makes easily the most audacious and original contribution. One need not pursue here his claim, brilliantly argued for, that scientific theories of sufficient scope and generality, as well as philosophical theories, cannot be falsified; nor his claim that inductive logic and confirmation theory cannot 'save' empiricism from the total scepticism into which Hume ('perhaps the first and last consistent empiricist') logically relapsed. For the main subject of both the 'Conversation' and Maxwell's 'Afterthoughts' was the role of scientific results in theories of mind and brain, and it is here that Maxwell capitalises on the current quite remote scientific picture of the external world in order to offer new reasons for adopting a physicalist view of the mind.

In brief, the argument goes like this. Contemporary science, rather than philosophical disquisition, makes the rejection of naïve realism mandatory and establishes the truth of the negative component of subjective idealism. Russell [1948] was right; 'knowledge by description' of the external world is always indirect and involves denoting features of entities concerning which we have no 'knowledge by acquaintance'. The paradigm of this epistemological limitation is also Russell's: what the operating neurosurgeon really sees is only a portion of his own brain. Our best scientific results to date thus provide knowledge merely of the *structural* properties of physical objects, states and events; the question as to what are their *intrinsic* properties must be left entirely open. If so, the possibility that some of the intrinsic properties of the external world are mental is also open. This is easier to accept if, again following Russell, we replace commonsense substance metaphysics with an event ontology. The concepts of 'things' or 'objects' become 'families of events'. We can then say that some of the events that *are among the constituents of the brain are mental events* (p. 353). And since as Russell noted elsewhere ([1959]), there is no more difficulty about saying that a given event is both mental and physical than there is in saying that a given man is both a baker and a father, we may conclude that all events are physical and none the less so just because some of them are mental (p. 354).

The whole argument seems to turn on the Russell–Maxwell notion of our 'seeing' not the world but our own brain. On this point there occurs an interesting exchange between Maxwell and Keith Gunderson in the printed 'Conversation' preceding Maxwell's 'Afterthoughts' (p. 322):

> *Gunderson:* As Maxwell presents the Russellian twist, we don't really see the physical object, the [patient's] brain, at all; what we think we see is a portion of our own brain. Now, I think that move is crucial. Do you want to generalize that across sensory modalities, so that when you hear things, you only hear portions of your brain? I am not sure how well that works. Do we smell a portion of our own brains?
> *Maxwell:* If you are hung up on ordinary language, of course "seeing" a portion of our own brain is problematic. You have to recognize that words like "see" and "hear" are *naive-realist* terms that presuppose certain commonsense beliefs about perception. But these beliefs turn out to be false, so the word "see" with its ordinary meaning is no longer applicable. When I say I see part of my own brain, of course this is no longer accurate, since in reality I don't *see anything* in the *ordinary* sense of the word. But the visual experience involved *is* an event that is a constituent of my brain, and *nothing* that is not a part of my brain is an ingredient of my visual experience, although other things or events including the physical object that we mistakenly *think* that we see *are* causal ancestors of the visual experience. It is in such a new but related sense of "see" that we *see* (parts (of our own brain. The same applies, full-force, to *hearing* and *smelling*.

What seems to have escaped Maxwell entirely (and Russell, too, if Maxwell is interpreting him correctly), is that the brain, including my own brain, is just as much an object in the external physical world as any other. It is by overlooking this fact that one is led into what might be called 'The I-am-a-Prisoner-of-my-Brain Fallacy'. For example, the neurophysiologist Vernon B. Mountcastle writes ([1975]):

> Each of us lives within the universe—the prison—of his own brain. Projecting from it are millions of fragile sensory nerve fibers, in groups uniquely adapted to sample the energetic states of the world around us: heat, light, force, and chemical composition. *That is all we ever know of it directly; all else is logical inference.* Sensory stimuli reaching us are transduced at peripheral nerve endings, and neural replicas of them dispatched brainward, to the gray mantle of the cerebral cortex. We use them to form dynamic and continually updated neural maps of our place and orientation in the external world, and of events within it. At the level of sensation, your images and my images are virtually the same, and readily identified one to another by verbal descriptions, or common reactions. Beyond that, each image is conjoined with genetic and stored experiential information that makes each of us uniquely private. From that complex integral each constructs at a higher level of perceptual experience, on my view in brain regions like those of the parietal lobe, his own, very personal, *view from within* (p. 131).

Now how does Mountcastle know this? The only answer must be, by observing and studying the brain and nervous system through microscopes, oscilloscope recordings after microelectrode implantation, *etc.* But then our knowledge of the brain, like our knowledge of objects external to the body, is equally inferential from our experience of it. So we do not know it 'directly' after all. In Russellian terminology, the brain—any brain at all—is a 'family of events' concerning which we have no 'knowledge by acquaintance' of its intrinsic properties, but only 'knowledge by description' of its structural properties. This means, *contra* Maxwell, that there can be no warrant for saying that my mental events are some of the events *among the constituents of my brain.* My brain is in the same epistemological boat as yours, and as that table over there. When the neurosurgeon looks at the patient's brain he is not 'really' seeing a portion of his own brain but *whatever it is out there* that causes him to see the patient's brain, and it is only by inference that he can suppose a portion of his own brain mediates, rather than constitutes, the visual experiences he is having.

To be consistent, then, Maxwell would have to stop where he started, namely with the observation that contemporary science requires a retreat to the negative stand of subjective idealism. Any claim to knowledge beyond what we experience has to be inferential and indirect, and that includes knowledge about the constituents of one's own brain. If he wants, of course, Maxwell can then go on to *assert* that mental events are just some of the constituents of one's brain, but he cannot *assume* this in order to get to a physicalist theory of the mind without begging the question.

ROLAND PUCCETTI
Dalhousie University

REFERENCES

Deeke, L., Scheid, P. and Kornhuber, H. H. [1969]: 'Distribution of the Readiness Potential, Pre-motion Positivity, and Motor Potential of the Human Cerebral Cortex Preceding Voluntary Finger Movements', *Experimental Brain Research*, **7**, pp. 156–68.

Llinás, R. R. [1975]: 'The Cortex of the Cerebellum', *Scientific American*, **232**, pp. 56–71.

Mountcastle, V. B. [1975]: 'The View from Within: Pathways to the Study of Perception', *The Johns Hopkins Medical Journal*, **136**, pp. 109–31.

Penfield, W. and Rasmussen, T. [1950]: *The Cerebral Cortex of Man: A Clinical Study of Localization.*

Penfield, W. and Roberts, L. [1959]: *Speech and Brain Mechanisms.*

Puccetti, R. [1977]: 'Sperry on Consciousness: A Critical Appreciation', *Journal of Medicine and Philosophy*, **2**, pp. 127–44.

Russell, B. [1948]: *Human Knowledge: Its Scope and Limits.*

Russell, B. [1959]: *My Philosophical Development.*

Sperry, R. W. [1977]: 'Forebrain Commissurotomy and Conscious Awareness', *Journal of Medicine and Philosophy*, **2**, pp. 101–26.

THE DEVELOPMENT OF MATHEMATICS*

1 *Introduction: General Assessment of the Book.*
2 *The Interest of History of Mathematics.*
3 *Major Trends in Mathematics and their Explanation.*
4 *Mathematics and its Applications.*
5 *Some Criticisms of Kline.*

1 INTRODUCTION: GENERAL ASSESSMENT OF THE BOOK

It is a rare pleasure for a reviewer to deal with a book as good as Kline's *Mathematical Thought from Ancient to Modern Times.* The work is obviously the product of years of research and thought on the subject. It will undoubtedly become the standard history of mathematics of its length, and deservedly so. That is not to say of course that there are not several points on which it is possible to disagree with Kline. I shall consider some of these later in the review, but first let me survey some of the excellent features of the book.

The first qualities to be looked for in a history of mathematics are a judicious selection of the most important results, a good exposition of these results, and a clear account of the chronological development. On these points Kline can hardly be faulted. His book is 1,238 pages, but when one considers the material covered (from the Babylonians to Gödel), it is really a masterpiece of concision.

As an example of Kline's method of exposition, we can consider his treatment of Euclid's Elements (ch. 4, pp. 57–88[1]). After a brief sketch of the background to the Elements, Kline goes on to give a summary of its contents. He lists the most important definitions and theorems, and, occasionally also gives a sketch of a proof to illustrate methods of proof. At each stage he makes some interesting comments on the significance of the results. Thus he points out that Euclid

---

*A review of M. Kline, [1972]: *Mathematical Thought from Ancient to Modern Times.* Oxford University Press. £23.25. Pp. 1238.

[1] All references to Kline will be to the work under review unless otherwise stated.

assumes only a collapsible compass, and therefore has to show how (Book I, Proposition 2) 'to place at a given point (as an extremity) a straight line equal to a given straight line'. Kline goes on to point out the nature and importance of the 'geometrical algebra' at the end of Book I and in Book II of the Elements. He explains the logical significance of Eudoxus's theory of magnitude given in Book V, and gives reasons why Euclid in his treatment of natural numbers (Books VII, VIII and IX) proceeds from first principles—rather than simply regarding number as a magnitude and using Book V. Finally in his account of Books XI, XII and XIII, Kline examines the logical basis of the Greek method of exhaustion—illustrating his account with a sketch of the proof of Book XII. Proposition 2: 'Circles are to one another as the squares on the diameters'. Kline concludes his account with a section (10, pp. 86–8) on 'The Merits and Defects of the Elements'. Here he lists some of the logical gaps in Euclid which have been discovered in the last century—for example Euclid's unconscious assumptions concerning the continuity of lines and circles. Most of Kline's points regarding the Elements are fairly familiar to historians of mathematics, but his treatment is remarkable for its clarity and conciseness.

Let us take one other example from a different historical period to illustrate Kline's methods of exposition. He gives an account of James Bernoulli's work on infinite series published as a supplement to the *Ars Conjectandi* in 1713 (ch. 20, section 4, pp. 442–4). Kline remarks (pp. 442–3):

> ... some of the methods he (James Bernoulli—D.G.) used to sum series are worth noting because they illustrate the nature of mathematical thought in the eighteenth century.

Kline gives two proofs of Bernoulli's that the harmonic series

$$1+\tfrac{1}{2}+\tfrac{1}{3}+\ldots+\frac{1}{r}+\ldots$$

is divergent. He considers some results of Bernoulli's for the series

$$1+\frac{1}{2^n}+\frac{1}{3^n}+\ldots+\frac{1}{r^n}+\ldots,$$

where n can be an integer $\geqslant 2$, or 1, or $\tfrac{1}{2}$, and also Bernoulli's discussion of the series $1-1+1-1+\ldots$. Kline observes (p. 444) that: '... James Bernoulli does many things so loosely that it is difficult to believe he had ever recognized the need for caution with infinite series.' By means of well-selected examples, it does seem to me that Kline succeeds in conveying something of the flavour of eighteenth-century analysis in a few brief pages.

A purist might object to the fact that Kline uses modern notations (and sometimes modern concepts) in his account of older results. However in my view Kline was quite right to do so. A use of antiquated notations would render the history much harder to understand, and obscure the development of ideas which Kline wants to make clear. Moreover Kline does devote a few pages here and there to describing older notations—for example chapter 13, section 3, pp. 259–63.

To form an assessment of Kline's work, it should perhaps be compared and contrasted with other histories. We will confine ourselves to considering the general history which is undoubtedly most comparable to Kline's: that is, Boyer's [1968]. Kline was a friend and colleague of Boyer's, and has written a generous obituary notice of Boyer in *Historia Mathematica*, 3 (1976) pp. 387–94. Here Kline acknowledges his debt to Boyer, and he also frequently cites Boyer's works in the Bibliographies at the end of the chapters of Mathematical Thought. It is therefore very interesting to compare Boyer's general history with Kline's.

In the period from early beginnings to 1700, the two books are fairly similar. Kline covers this period in roughly 400 pages, and Boyer in roughly 450. My own judgment is that Boyer's account is more readable, but that Kline's, although slightly shorter, contains more mathematical detail. Thus Kline's treatment of Euclid, which we described earlier is quite similar to that given by Boyer ([1968] ch. VII, pp. 111–31). However Kline gives a detailed description of Euclid's method of exhaustion which is only briefly mentioned by Boyer. Or again Kline gives a full description of some important 'projective' results of Pappus (pp. 127–8) which are not mentioned by Boyer. Boyer, however, does discuss briefly tribal, Mayan and Chinese mathematics—topics which are not mentioned by Kline.

The real structural difference between Kline and Boyer, however, occurs in their treatment of the period 1700 to the present. Kline covers this period in roughly 800 pages, whereas Boyer devotes not much more than 220 pages to it. Obviously Kline can enter into much greater detail than Boyer, and indeed he describes at length whole topics (*e.g.* differential equations, complex function theory) which Boyer only briefly mentions.

This structural difference between Kline's history and Boyer's is fairly obvious, but there is also a less obvious but very important difference in outlook between the two authors. We can sum it up by saying that Boyer favours pure, and Kline applied, mathematics. However, this is a topic to which we shall return later in the review.

It would scarcely be possible in a review of manageable proportions to summarise every topic which Kline discusses in his long history. It seems to me both more practical and more interesting to discuss a few general themes which emerge from Kline's work. In fact I have selected three, and will deal with them in the next three sections. They concern first the general problem of why the history of mathematics may be worth studying, second the question of what major trends can be detected in the history of mathematics, and how these might be explained, and third the problem of the relationship between mathematics and its applications. In each case I shall try to expound Kline's views and compare them to those of other authors—sometimes adding a few comments of my own.

In the final section of the review I shall adopt a more critical tone, and discuss a number of parts of Kline's work which seem to me either wrong or inadequate in some way. This section will be concerned with probability, determinants and matrices, and non-standard analysis.

2 THE INTEREST OF HISTORY OF MATHEMATICS

Kline has undertaken his study of history mainly because he thinks it will be useful to mathematicians. As he says himself (Preface, p. viii): 'I hope that this work will be helpful to professional and prospective mathematicians.' There are two ways in which Kline believes that the study of history can be helpful.

First of all it can combat the dangers which arise from the recent proliferation of mathematics into a multitude of special branches which seem to have little to do with each other, and many of which appear to be of doubtful value. Kline believes that history can provide a unifying theme and help to direct research towards important problems. As he himself puts it (pp. viii and ix):

> . . . mathematics, despite the proliferation into hundreds of branches, is a unity and has its major problems and goals. Unless the various specialties contribute to the heart of mathematics they are likely to be sterile. Perhaps the surest way to combat the dangers which beset our fragmented subject is to acquire some knowledge of the past achievements, traditions, and objectives of mathematics so that one can direct his research into fruitful channels.

and again (p. ix):

> The usual courses present segments of mathematics that seem to have little relationship to each other. The history may give perspective on the entire subject and relate the subject matter of the courses not only to each other but also to the main body of mathematical thought.

I cannot but agree with Kline regarding the problem of the fragmentation of modern mathematics, and the dangers that this will lead to trivial research. However it is to be doubted whether the study of history will provide more than part of the answer to this problem. This is a question to which we will return later in the review.

Kline believes that history may also help the student to understand the subject better, and encourage the researcher to make new contributions. Kline feels that the elegant deductive presentations of mathematics to be found in the usual textbooks and courses are misleading in a number of ways. They obscure the gaps and difficulties that still remain in the subject, and also the great problems that had to be overcome in creating the subject. Yet knowledge of these matters gives a deeper understanding and a stimulus to creating something new. As Kline puts it (p. ix):

> The usual courses in mathematics are also deceptive in a basic respect. They give an organized logical presentation which leaves the impression that mathematicians go from theorem to theorem almost naturally, that mathematicians can master any difficulty, and that subjects are completely thrashed out and settled. The succession of theorems overwhelms the student, especially if he is just learning the subject. . . .
>
> The polished presentations in the courses fail to show the struggles of the creative process, the frustrations, and the long arduous road mathematicians must travel to attain a sizable structure. Once aware of this, the student will not only gain insight but derive courage to pursue tenaciously his own problems and not be dismayed by the incompleteness or deficiencies in his own work. Indeed the account of how mathematicians stumbled, groped their way through obscurities, and arrived piecemeal at their results should give heart to any tyro in research.

As regards both these points, Kline has an ally in Lakatos. Indeed Lakatos speaks bluntly ([1963–4], p. 329) of 'the tide of pretentious trivialities in mathematical literature'—though Lakatos recommends as a remedy, not so much the study of history, as (*op. cit.* p. 329): '. . . mathematical critics just as you have literary critics, to develop mathematical taste . . .'. Again we may doubt whether this remedy, even if practical, would prove effective.

Lakatos also gives some striking examples of how the study of history might help the student to a better understanding of mathematics. Lakatos cites the following definition of polyhedron (*op. cit.* p. 17): 'a system of polygons arranged in such a way that (1) exactly two polygons meet at every edge and (2) it is possible to get from the inside of any polygon to the inside of any other polygon by a route which never crosses any edge at a vertex.'

Lakatos adds in a footnote (*op. cit.* p. 17, note 1) that this 'cumbersome definition (is) reproduced in some modern textbooks in the usual authoritarian "take it or leave it" way; . . .'. Indeed, if the definition is produced out of the blue, it appears quite unintelligible, whereas, if the historical background to its formulation is sketched (as Lakatos has done in the preceding pages), the definition becomes quite natural.

Kline then holds that the study of history of mathematics should prove useful to mathematicians. There is, however, another group to whom it should be valuable: namely philosophers of mathematics. Indeed if we consider the three schools in philosophy of mathematics which emerged and developed in the half-century *c.* 1880–*c.* 1930: logicism, formalism, and intuitionism, it is striking how negligent all three are of history.

Both logicism and formalism take mathematics as something given, and see their task as being that of justifying mathematics. This of course they try to do in different ways: logicism by reducing mathematics to logic, and formalism by reducing it to formal systems which are then proved consistent using a finitist metamathematics. However both schools tend to forget that mathematics is not something eternal and unchanging. In fact what they take to be 'mathematics' is really late nineteenth-century analysis and number theory, together with some parts at least of Cantorian set theory. Now this body of mathematics is very different from the mathematics of the ancient Greeks, or even of the seventeenth and eighteenth centuries. Indeed the mathematics of today is very different from that which Russell and Whitehead attempted to justify in *Principia Mathematica* (1910–13). Few of the topics of a modern mathematics course are mentioned in that work. The general point is that mathematics 'is no solid crystal, but an organism capable of change, and is constantly changing'.

The intuitionists have the merit that they think of mathematics as growing and developing, but they conceive of this development in a quite unhistorical way. Thus Brouwer imagines a solitary mathematician creating the number system in his own mind from his own basal intuition of the 'bare two-oneness'. In fact, however, the creation of the number system was a long and painful historical process to which many hundreds of individuals contributed. The illusion that a single individual can do it himself in his own consciousness only arises because the results of this long historical process are taught at school.

The whole matter can be made clearer if we compare the situation in the philosophy of mathematics with that in the philosophy of science. In the heyday of logical positivism some, at least, of that school thought that the task of philosophy of science was to analyse the logical structure of science using formal techniques. However the main works in philosophy of science published since the war (those of Feyerabend, Kuhn, Lakatos and Popper) lay great stress on science's historical growth, development, and revolutionary transformations. For the most part, however, philosophers of mathematics have not adopted this historical approach. Of course the great exception is Lakatos himself who writes in the introduction to 'Proofs and Refutations' (*op. cit.* p. 3): '. . . the philosophy of mathematics, turning its back on the most intriguing phenomena in the history of mathematics, has become *empty*.' Let us hope that Kline's work, and the recent publication of Lakatos's 'Proofs and Refutations' in book form will help philosophers to take a more historical view of mathematics.

3 MAJOR TRENDS IN MATHEMATICS AND THEIR EXPLANATION

If a historian is going to be more than a mere empiricist chronicler of facts, he must seek to pick out major trends, and also try (though this is more difficult) to uncover the causes of these trends. Kline does indeed attempt both tasks. In his account of the early period in the history of mathematics—that is, the period from first beginnings to the Renaissance, Kline points out two different traditions in the development of mathematics. This is how he describes them (p. 199):

> Two independent traditions or concepts of mathematics had now become established: on the one hand, the logical deductive body of knowledge that the Greeks established, which served the larger purposes of understanding nature; and on the other, the empirically grounded, practically oriented mathematics founded by the Egyptians and Babylonians, resuscitated by some of the Alexandrian Greeks, and extended by the Hindus and Arabs. The one favored geometry and the other arithmetic and algebra. Both traditions and both goals were to continue to operate.

One important difference between the two traditions is that the arithmetic–algebraic school conceived of mathematics as a series of calculating techniques justified practically, and did not evolve any notion of proof. Conversely the geometrical school developed the deductive/axiomatic presentation of mathematics.

Thus speaking of Babylonian mathematics, Kline writes (p. 14):

> Almost surely, the arithmetic and algebraic processes and the geometrical rules were the end result of physical evidence, trial and error, and insight. That the methods worked was sufficient justification to the Babylonians for their continued use. The concept of proof, the notion of a logical structure based on principles warranting acceptance on one ground or another, and the consideration of such questions as under what conditions solutions of problems can exist, are not found in Babylonian mathematics.

and again, regarding the Hindus (p. 190):

> As our survey indicates, the Hindus were interested in and contributed to the arithmetical and computational activities of mathematics rather than to the deductive patterns. Their name for mathematics was *ganita*, which means "the science of calculation."

Kline goes on to point out that (p. 198) the Hindus and Arabs:

> worked freely in arithmetic and algebra and yet did not concern themselves at all with the notion of proof

even though, unlike the Egyptians and Babylonians, they (p. 198)

> were aware of the totally new concept of mathematical proof promulgated by the Greeks.

This raises the question of why the Greeks introduced the new concept of deductive mathematics in what Kline calls the classical period 600–300 B.C. This trend culminated in Euclid's Elements, but the main steps were undoubtedly taken earlier by the Pythagoreans, the Eleatics, and the members of Plato's academy. Kline gives the following tentative explanation for the emergence of deductive mathematics in the classical period (pp. 45–6):

> Still another reason for the Greek preference for deduction may be found in the contempt shown by the educated class of the classical Greek period towards practical affairs. Though Athens was a commercial center, business as well as such professions as medicine were carried on by the slave class. Plato contended that the engagement of freemen in trade should be punished as a crime, and Aristotle said that in the perfect state no citizen (as opposed to slaves) should practice any mechanical art. To thinkers in such a society, experimentation and observation would be alien. Hence no results, scientific or mathematical, would be derived from such sources.

As a matter of fact this passage contains a number of historical errors. It is true that in Athens most of the commerce and industry was in the hands of a special class of metics (resident aliens) who were forbidden to purchase land (*cf.* A. Andrewes [1967], p. 97). However the metics were not slaves. Moreover many of the aristocratic landowners engaged in such commercial activities as selling agricultural produce and purchasing slaves. Aristotle's father was a doctor, but certainly not a slave.

Moreover even if we grant to Kline that classical Athens contained an aristocratic stratum who did not engage in business or commerce, why should such a stratum produce deductive mathematics? Such an aristocratic stratum existed at other historical periods (*e.g.* the Middle Ages) without taking any interest in mathematics. Another unexplained point is why the shift occurred from arithmetic and algebra to geometry, and why it was that, until the last century, the deductive approach was associated with geometry rather than algebra. Geometry is not inherently more deductive than algebra, as modern abstract algebra shows.

One interesting feature of Kline's account is the distinction he draws between Greek mathematics of the classical period 600–300 B.C. and of the Alexandrian period 300 B.C. to A.D. 600. Euclid, though he lived in Alexandria, is classified as 'classical' because his work organizes the results and methods of the classical period. Here is Kline's description of how Alexandrian geometry differed from classical (p. 104):

> Alexandrian geometry was devoted in the main to results useful in the calculation of length, area, and volume. It is true that some such theorems are also in Euclid's *Elements*. For example, Proposition 10 of Book XII asserts that any cone is a third part of the cylinder which has the same base and height. Hence if one knows the volume of a cylinder he can compute the volume of a cone. However, such theorems are relatively scarce in Euclid, whereas they are the major concern of the Alexandrian geometers. Thus, while Euclid was content to prove that the areas of two circles are to each other as the squares on their diameters—which leaves us with the knowledge that $A = kd^2$ but without a value of k—Archimedes obtained a close approximation to π so that circular areas could be computed.

Kline goes on to say that the Alexandrians introduced trigonometry, and revived and extended arithmetic and algebra. How are these changes in the character of Greek mathematics to be explained? Kline develops his earlier explanation as follows (pp. 49–50):

> The question also arises: What did the classical Greeks do about the need for numbers in scientific work and in commerce and other practical affairs? Classical Greek science, as we shall see, was qualitative. As for the practical use of numbers, we mentioned earlier that the intellectuals of that period confined themselves to philosophical and scientific activities and took no hand in commerce or the trades; educated people did not concern themselves with practical problems. But one could think about all rectangles in geometry without concerning himself in the least with the actual dimensions of even one rectangle. Mathematical thought was thus separated from practical needs, and there was no compulsion for the mathematicians to improve arithmetical and algebraic techniques. When the barrier between the cultured and slave classes was breached in the Alexandrian period (300 B.C. to about A.D. 600) and educated men interested themselves in practical affairs, the emphasis shifted to quantitative knowledge and the development of arithmetic and algebra.

This is an elegant theory, but unfortunately it is highly doubtful historically whether the barriers between slave and 'cultured' classes were lower in the Alexandrian than the classical period.

Similar difficulties beset Kline's explanation of why the Hindus and Arabs preferred the tradition of arithmetic, algebra and calculation to that of geometry and deduction. Kline writes (p. 198):

> Both the Hindus and Arabs favored arithmetic, algebra, and the algebraic formulation of and operation with trigonometric relationships. This predisposition may bespeak a different mentality or it may reflect a response to the needs of the civilizations. Both of these civilizations were practically oriented and, as we have had occasion to note in connection with the Alexandrian Greeks, practical needs do call for quantitative results, which are supplied by arithmetic and algebra.

But is it really true that Hindu and Arab civilisations were more 'practically oriented' than that of classical Athens? What even can be meant by such a claim? There can be no doubt that a lot of 'practical activities' of various kinds went on in classical Athens.

As for the 'different mentality' theory, this is apt to become circular. The Greeks developed geometry because they had a geometrical mentality, and the proof of their having a geometrical mentality is that they developed geometry.

The next major problem which confronts a historian of mathematics is why mathematics which had flourished so brilliantly under the Greeks was hardly cultivated at all by the Romans or by western Europeans during the Middle Ages. We cannot explain this merely on the grounds of the breakdown of civilisation and the subsequent Dark Ages, because both the Romans and the medieval Europeans were, at certain periods at least, highly civilised and produced remarkable cultural achievements in other fields *e.g.* art, architecture and literature. This is what Kline has to say about the matter (p. 204):

> The Roman civilization was unproductive in mathematics because it was too much concerned with practical and immediately applicable results. The civilization of medieval Europe was unproductive in mathematics for exactly the opposite reason. It was not at all concerned with the physical world. Mundane matters and problems were unimportant. Christianity put its emphasis on life after death and on preparation for that life.

Again historical facts raise problems for Kline's explanation. The Romans were religious and superstitious as well as practical. Mystery religions flourished under the Roman empire, and it was in this atmosphere that Christianity itself developed. Conversely it is incorrect to say that medieval civilisation 'was not at all concerned with the physical world'. Actually many more practical and technological innovations occurred during the medieval period than under the Roman empire. We may mention (*inter alia*): improved agricultural technique giving higher yields than were possible in the ancient world, improved horse harness, improved ship-building (including the stern-post rudder), the compass, clocks, improved glass making, and the invention of spectacles, distillation, better cloth-making and dyeing techniques *etc.* Yet despite all this technological advance, there was little interest in, or development of, mathematics in the Middle Ages.

I think nonetheless that it may be possible to revive Kline's theory in a slightly changed form. What we might postulate is that for mathematics to flourish, it is necessary for there to be a group within society who (*i*) have sufficient leisure and education to carry on mathematical research, but who (*ii*) have sufficient involvement in practical and commercial affairs to be interested in mathematics. In the Middle Ages the peasants and artisans had practical knowledge, but no leisure or education. The monks had leisure and education but no involvement in practical affairs. Thus mathematics revives at the end of the Middle Ages when a merchant class appears that combines both practical interests with some degree of leisure and education.

It is worth noting that, if this theory is to work, we must postulate that the Athenian aristocracy developed mathematics because they had *more* involvement in commercial life than most aristocracies. This is the opposite of what Kline claims, but I believe that a good historical case can be made for this view—though I shall not try to do so here.

The above theory seems to me along the right lines, but, as just sketched, it is woefully inadequate, and needs considerable development. Not to mention other matters, it fails to explain the two traditions: geometry/deduction, and arithmetic/calculation, or the differences between classical and Alexandrian mathematics.

Let us now turn to the modern period (the Renaissance to the present day) and see what major trends Kline discovers in mathematics during this time. The first thing that strikes one about the period *c.* 1500–*c.* 1800 is that, although the classic Greek texts on geometry (Euclid and Apollonius) were translated and admired, the mainstream of mathematical research did not follow the Greek geometry/deduction tradition, but rather the Hindu–Arab algebra/calculation tradition. The situation is a curious one for the Greeks are praised in words, but their example is not followed in practice. Actually just the same is true of Renaissance art. The Renaissance artists all praise the wonders of Greek and Roman art, but the great majority of Renaissance paintings deal, nonetheless, with Christian religious subjects, *e.g.* the Madonna and Child, and constitute a revolutionary development of medieval art.

The first major triumph of Renaissance mathematics was of course the algebraic solution of third and fourth degree equations by dal Ferro, Tertaglia, Cardano and Ferrari (Kline, pp. 263–70). This of course is a development in algebra rather than geometry. However the decisive triumph of the algebraic tradition came with Descartes and coordinate geometry which effectively subordinated geometry to algebra. Kline puts the matter as follows (p. 323):

> In fact, coordinate geometry paved the way for a complete reversal of the roles of algebra and geometry. Whereas from Greek times until about 1600 geometry dominated mathematics and algebra was subordinate, after 1600 algebra became the basic mathematical subject; . . .

Actually Kline makes a slight slip when he speaks of geometry dominating mathematics from Greek times until about 1600 for he himself, as we have seen, stresses that algebra was the most important part of Hindu and Arabic mathematics. However the main point is certainly correct. Whereas in Greek mathematics geometry dominated algebra—even in the Alexandrian period, in early modern times the roles were reversed, and the invention of coordinate geometry subordinated geometry to algebra.

Some objections might be raised to this view but they are effectively answered by Kline. First of all it could be objected that synthetic projective geometry was developed in the seventeenth century by Desargues, Pascal and La Hire. Kline does describe this development in chapter 14, pp. 285–301, but he points out that it was largely ignored by contemporaries and stood outside the mainstream of mathematical development (p. 301):

> But pure mathematics was not congenial to the temper of the seventeenth century, whose mathematics were far more concerned with understanding and mastering nature—in short, with scientific problems. Algebraic methods of working with mathematical problems proved in general to be more effective, and in particular to yield the quantitative knowledge that science and technology needed. The qualitative results the projective geometers

> produced by their synthetic methods were not nearly so helpful. Hence projective geometry was abandoned in favor of algebra, analytic geometry, and the calculus, which themselves blossomed into still other subjects of central importance in modern mathematics. The results of Desargues, Pascal and La Hire were forgotten and had to be rediscovered later, chiefly in the nineteenth century . . .

and again (p. 392):

> The work of Desargues, Pascal and La Hire was depreciated and forgotten, and the geometric methods of Cavalieri, Gregory of Saint Vincent, Huygens, and Barrow were superseded. Pure geometry was eclipsed for about a hundred years, becoming at best a guide to algebraic thinking through coordinate geometry. . . . What was evident by 1700 was explicitly stated by no less an authority than Euler, who, in his *Introductio in Analysin Infinitorum* (1748), praises algebra as far superior to the synthetic methods of the Greeks.

Another objection might be that Newton used pure geometry in the *Principia*, and praised the geometrical methods of the ancients. This is true of course. However Newton's principal successors did not follow him in the use of geometry, but rather gave Newtonian mechanics an analytic formulation. Once again the mainstream eschewed geometry. Moreover, even Newton, in his other mathematical works contributed to the triumph of algebra and analysis, as Kline acutely observes (p. 392):

> According to Henry Pemberton (1694–1771), who edited the third edition of Newton's *Principia*, Newton not only constantly expressed great admiration for the geometers of Greece but censured himself for not following them more closely than he did. In a letter to David Gregory, Newton remarked that "algebra is the analysis of the bunglers in mathematics". But his own *Arithmetica Universalis* of 1707 did as much as any single work to establish the supremacy of algebra. Here he set up arithmetic and algebra as the basic science, allowing geometry only where it made demonstrations easier.

A final objection might be that pure geometry was pursued by English mathematicians in the eighteenth century. However, as Kline remarks (p. 392): '. . . their contributions were trivial compared to what the Continentals were able to achieve using the analytical approach.' In effect the English mathematicians in the eighteenth century had cut themselves off from the mainstream of mathematics which, during the course of the century, became ever less geometrical. Perhaps the lowest point for geometry was reached with the publication of Lagrange's *Analytical Mechanics*. Lagrange wrote in the preface (quoted from Kline p. 615):

> No diagrams will be found in this work. The methods which I expound in it demand neither constructions nor geometrical or mechanical reasonings, but solely algebraic [analytic] operations subjected to a uniform and regular procedure.

Given this development it is a surprising and striking fact that pure geometry was revived during the period of the French Revolution and became again a mainstream mathematical subject during the first half of the nineteenth century. The main initiator of the geometrical revival was Monge who became professor at the Ecole Polytechnique and founded a school of geometers. According to Kline the effect of Monge's views was (p. 565) 'to put geometry on at least an equal basis with analysis and then to inspire the revival of pure geometry. He is the first real innovator in synthetic geometry after Desargues'.

During the first half of the nineteenth century pure geometry was developed not only in France, but also in Germany and Eastern Europe. Curiously as pure geometry revived on the continent, the English lost interest in it, and turned to algebra instead. Kline gives an account of nineteenth-century developments in pure geometry in chapters 35 and 36. We will mention a few results.

In western Europe research was concentrated on Euclidean geometry and projective geometry. Regarding the former, the theorem of the nine-point circle was established by Gergonne and Poncelet in 1820/1, and independently, by Feuerbach, in 1822. Another striking result concerned construction methods (Kline, p. 840):

> In his *Traité* of 1822, Poncelet showed that all constructions possible with straightedge and compass (except the construction of circular arcs) are possible with straightedge alone provided that we are given a fixed circle and its center. Steiner reproved the same result more elegantly in a small book . . . The Geometrical Constructions Executed by Means of the Straight Line and a Fixed Circle (1833).

Monge's school were the main force behind the revival of projective geometry. Carnot, a pupil of Monge, wrote two books on the subject in 1803 and 1806. Brianchon, while a student at the Ecole Polytechnique, proved his famous theorem (the dual of Pascal's theorem) (1806). However, Kline (pp. 841–2):

> The revival of projective geometry received its main impetus from Poncelet (1788–1867). Poncelet was a pupil of Monge and he also learned much from Carnot. While serving as an officer in Napoleon's campaign against Russia, he was captured and spent the year 1813–14 in a Russian prison at Saratoff. There Poncelet reconstructed without the aid of any books what he had learned from Monge and Carnot and then proceeded to create new results. He later expanded and revised this work and published it as the *Traité des propriétés projectives des figures* (1822). This work was his chief contribution to projective geometry and to the creation of a new discipline.

In his book Poncelet developed the principle of continuity (already introduced by Monge and Carnot). He introduced the circular points at infinity, and studied the relations of pole and polar. He is also one of the inventors of the principle of duality, but, as he tended to confuse this with the pole–polar relationship, the credit for duality should perhaps go to Gergonne.

Yet more remarkable than these western European developments was the introduction of non-Euclidean geometry in eastern Europe. The first published accounts were by Lobatchevsky (1829) and Bolyai (1832).

The nineteenth-century geometrical impulse seems to have been largely spent by 1870. As Kline says (p. 921):

> The interest in the classical synthetic non-Euclidean geometries and in projective geometry declined after the work of Klein and Lie partly because the essence of these structures was so clearly exposed by the transformation viewpoint. The feeling of mathematicians so far as the discovery of additional theorems is concerned was that the mine had been exhausted. The rigorization of the foundations remained to be accomplished and this was an active area for quite a few years after 1880.

Kline describes this late nineteenth-century work on the foundations of geometry in chapter 42. The most famous essay of that period was of course Hilbert's Foundations of Geometry which deals with Euclidean geometry. Numerous axiom systems for projective geometry were also produced in the period 1889–1910. However by 1910 these investigations had, in turn, petered out. Since then, pure geometry has virtually disappeared as an area of mathematical research. Such branches of modern mathematics as do have a geometrical character (*e.g.* topology and differential geometry) are treated almost exclusively by algebraic and analytic methods.

Regarding the two main mathematical traditions—the algebraic, and the geometrical, another striking development occurred in the nineteenth century. Geometry had been cast into an axiomatic form by the Greeks, but arithmetic and algebra were, up till the nineteenth century, developed as calculating techniques. Only in the nineteenth century is there an attempt, for the first time, to develop these subjects deductively. The first steps in this direction were taken by the British school of algebra (Peacock, De Morgan, Cayley, Hamilton, Sylvester and Boole) from the 1830s on. Then around 1870 came the important step of defining real numbers in terms of rational numbers (Weierstrass, Cantor, Dedekind, Heine and Méray). Finally in the 1880s the first attempts to develop arithmetic deductively were made (Frege, Dedekind and Peano). This historical fact raises an interesting question. After all the axiomatic-deductive approach was known and admired from Greek times. Why did mathematicians wait till the nineteenth century before attempting to apply it to arithmetic and algebra? As Kline puts it (p. 979):

> One of the most surprising facts in the history of mathematics is that the logical foundation of the real number system was not erected until the late nineteenth century. Up to that time not even the simplest properties of positive and negative rational numbers and irrational numbers were logically established, nor were these numbers defined.

and again (p. 1024):

> No matter what individual mathematicians may have thought about the soundness of their results, the fact is that from about 200 B.C. to about 1870 almost all of mathematics rested on an empirical and pragmatic basis. The concept of deductive proof from explicit axioms had been lost sight of. It is one of the astonishing revelations of the history of mathematics that this ideal of the subject was, in effect, ignored during the two thousand years in which its content expanded to extensively.

Moreover the attempt to axiomatise arithmetic brought to light hitherto unsuspected limitations on the axiomatic/deductive method (Gödel's theorems).

I will conclude this section by pointing out a difference between Kline's treatment of (*i*) the ancient and medieval periods (*i.e.* up to 1500) and (*ii*) the modern period (*i.e.* from 1500). In both cases Kline, as we have shown, gives a good account of the major trends in mathematics. In the period up to 1500 he gives some proposed explanations of these trends. These explanations are not always very satisfactory, but at least they are attempted. Kline does not, however, attempt any explanations for the major trends after 1500. Here then is a problem area which invites further investigation.

4 MATHEMATICS AND ITS APPLICATIONS

Let us turn to another general question—the relationship between mathematics and its applications. Which is to be preferred—pure or applied mathematics? Does mathematics develop better when pursued for its own sake or when closely tied to practical applications? It is interesting to note that Kline and Boyer give diametrically opposed answers to these questions. Boyer favours pure mathematics, and the pursuit of mathematics for its own sake, while Kline favours applied mathematics and the connection with practice. In fact Kline organised and directed for twenty years the Division of Electromagnetic Research at the Courant Institute of Mathematical Sciences at New York University. So he might be expected to favour the applied side. Boyer, on the other hand, was more of a pure academic. I shall now try to illustrate the difference between the two men with some quotations from their respective histories.

In his very first chapter on 'primitive origins' Boyer reveals his preference for pure mathematics as follows (Boyer [1968], p. 7):

> We would like to think that at least some of the early geometers pursued their work for the sheer joy of doing mathematics, rather than as a practical aid in mensuration; . . .

Moreover when discussing the period from Hipparchus to Ptolemy, Boyer explicitly states that too much concern for applications inhibits the development of mathematics (*op. cit.*, pp. 193–4):

> It sometimes is held that mathematics develops most effectively when in close touch with the world's work; but the period we have been considering would argue for the opposite thesis. The loss of nerve in religion and philosophy, which led the Greeks to pursue cults and mysticism, was paralleled in mathematics by a movement toward applications which persisted for more than three centuries. From Hipparchus to Ptolemy there were advances in astronomy and geography, optics and mechanics, but no significant developments in mathematics. . . . It is evident that the rapid growth of mathematics from Eudoxus to Apollonius, when theoretical considerations were in the forefront had come to an end. Perhaps the trend toward applications was the result of the decline, rather than its cause, but in any case the two were concomitant.

In contrast to all this, Kline frequently points out how practical applications stimulated mathematical developments. One interesting example is his discussion of the development of the function concept. Kline shows how the study of physical problems (vibrating strings, flow of heat) led to differential equations, and how attempts to solve these equations led, via such devices as Fourier series, to successive extensions of the concept of function. In discussing the motivation for eighteenth century work in mathematics, Kline writes (pp. 616–17):

> Far more than in any other century, the mathematical work of the eighteenth century was directly inspired by physical problems. . . . the mathematicians undertook technological problems as a matter of course. Euler, for example, worked on the design of ships, the action of sails, ballistics, cartography, and other practical problems. Monge took his work on excavation and fill and the design of windmill vanes as seriously as any problem of differential geometry or differential equations.

Again, speaking of the nineteenth century, Kline writes (p. 791):

> The influence of physics in stimulating the creation of such mathematical entities as quaternions, Grassman's hypernumbers, and vectors should be noted.

The contrast between Kline and Boyer comes out very clearly in their treatment of the invention of analytic geometry. Boyer writes ([1968], p. 379):

> Our account of Descartes' analytic geometry should make clear how far removed the author's thought was from the practical considerations that are now so often associated with the use of coordinates. He did not lay down a coordinate frame in order to locate points as a surveyor or geographer might do, . . . *La géométrie* was in its day just as much a triumph of impractical theory as was the *Conics* of Apollonius in antiquity, despite the inordinately useful role that both were ultimately destined to play.

Kline is very far from considering *La géométrie* 'a triumph of impractical theory'. Perhaps his clearest statement of the background to the work of Fermat and Descartes is contained in an earlier work ([1954], pp. 164–5):

> We must digress briefly to understand why it was that the great mathematicians of the time were so much concerned with the study of curves. . . . Ellipses, parabolas, and hyperbolas became important because they described the paths of the planets and comets. The motion of the moon was intensively studied to help locate ships at sea. The curved path of light rays through the atmosphere was of interest to astronomers and artists, while the curvature of lenses was studied for use in spectacles, the telescope and microscope, and for an understanding of the operation of the human eye.

This is a general account of why curves were of interest to mathematicians in the seventeenth century. In his [1972] Kline also gives a particular example relating to Descartes (p. 315):

> Descartes sought to design a lens that would focus the rays perfectly.
>
> He proceeded to solve the general problem of what surface should separate two media such that light rays starting from one point in the first medium would strike the surface, refract into the second medium, and there converge to one point. He discovered that the curve generating

> the desired surface of revolution is an oval, now known as the oval of Descartes. This curve and its refracting properties are discussed in *La Dioptrique*, and the discussion is supplemented in Book II of *La Géométrie.*

Kline, I would judge gets the better of this argument. It seems implausible to claim that *La Géométrie* is a 'triumph of impractical theory' when the study of curves had, at that time, so many known applications in different fields.

If Boyer errs, however, in claiming Descartes for pure mathematics, Kline seems to make a similar mistake in the opposite direction as regards Riemann. Thus he writes (pp. 655–6):

> Riemann is often described as a pure mathematician, but this is far from correct. Though he made numerous contributions to mathematics proper, he was deeply concerned with physics and the relationship of mathematics to the physical world.

Riemann may have been concerned with physics, but the fact remains that the great majority of his mathematical work (whether in complex function theory, number theory or on trigonometric series) is pretty far removed from practical applications.

The dispute between pure and applied mathematics is not just a matter of history. It vitally affects any assessment of the present state of mathematics. Since the beginning of this century pure mathematics has cut itself off more and more from practical applications. At the same time there has been an enormous increase in the number of research publications on pure mathematics. At least two different attitudes to this situation are possible. On the one hand the twentieth century can be seen as a golden age of mathematics in which pure mathematics, having at last shaken free the shackles of application, is undergoing an unprecedented expansion. This favourable view is adopted by Boyer. On the other hand it might be argued that the enormous growth of mathematical publications is a surface feature which actually conceals a deep and serious crisis in the subject. It could be held that the vast majority of these new results in pure mathematics are of very doubtful value, or even, as Lakatos puts it ([1963–4], p. 329), part of 'the tide of pretentious trivialities in mathematical literature'. This second view is, roughly speaking, Kline's. Kline proposes two remedies designed to produce more mathematical results of interest and importance. The first, as we saw earlier, is the study of history. The second is the establishment of closer links between mathematics and practical applications. Let us begin by sketching Boyer's position.

Significantly Boyer places at the head of his chapter on the twentieth century (ch. 27) the following motto from C. J. Keyser (Boyer [1968], p. 649):

> The golden age of mathematics—that was not the age of Euclid, it is ours.

Boyer himself writes (*op. cit.* p. 646):

> . . . in mathematical achievements already recorded, this century easily leads all the rest.

and again (*op. cit.* p. 656):

> . . . mathematicians . . . have continued to pile theorem upon theorem at a greater rate than ever before. Most assuredly no scholar of today would echo the assertion of Babbage in 1813 that 'The golden age of mathematical literature is undoubtedly past.'

Later on Boyer mentions some fears which Lebesgue expressed concerning the general trend of twentieth century mathematics. Boyer asserts firmly that these fears have proved to be quite unjustified (*op. cit.* p. 664):

> Although his concept of the integral was in itself a striking generalization, Lebesgue feared that, 'Reduced to general theories, mathematics would be a beautiful form without content. It would quickly die'. Later developments seem to indicate that his fears concerning the baneful influence of generality in mathematics were without foundation.

There is a very striking contrast between Boyer's rose-tinted view, and the sombre picture which Kline paints of present-day mathematics. At a recent conference on "Two hundred years of mathematics in America" at Oxford, Ohio on 3–4 October 1975, Kline devoted a paper entitled "The Break from Science" to what he obviously regards as a deep-seated crisis in modern mathematics. Here is how he summarises this paper (Kline [1975], pp. 463–4):

> There is no question that historically the major concern of mathematicians was the investigation of the laws of nature, and there is as little question that the major ideas of mathematics were derived from the efforts to formulate and treat scientific phenomena. But about 75 years ago, most mathematicians broke away from the science and began to investigate problems internal to mathematics. Some of these investigations are justified insofar as the value to science is concerned but most are not, and the trend is more and more towards the latter type. The value of most pure mathematics is dubious. Though the reasons for the break from science are rather forceful, nevertheless the break poses a threat to the life of mathematics and to its role in education unless some remedial measures are undertaken.

This is the point of view on present day mathematics which Kline adopts in his history. Thus he writes (p. 1039):

> However confident mathematicians of the last half-century may be that their work is of the utmost importance, the place of their contributions in the history of mathematics cannot be decided at the present time.

Kline is also fond of giving examples of branches of mathematics which aroused tremendous interest at a particular time, only to be forgotten later. His message is that oblivion may be the fate of much of twentieth century mathematics. Thus he says in the preface (p. viii):

> . . . the importance of many of the more recent developments cannot be evaluated objectively at this time. The history of mathematics teaches us that many subjects which aroused tremendous enthusiasm and engaged the attention of the best mathematicians ultimately faded into oblivion. One has but to recall Cayley's dictum that projective geometry is all geometry, and Sylvester's assertion that the theory of algebraic invariants summed up

> all that is valuable in mathematics. Indeed one of the interesting questions that the history answers is what survives in mathematics. History makes its own and sounder evaluations.

In the final chapters of his history (chs. 44–51), Kline considers the development of various branches of mathematics in the twentieth century (up till roughly 1930). Here he expresses frequent doubts about the value of recent developments (see, for example, his remarks on abstract algebra (p. 1157) and point set topology (pp. 1162–3)).

Opinions will obviously differ as to which of the two general views regarding present day mathematics is correct, but it seems to the present writer that Kline's gloomy picture is more truthful than Boyer's cheerful one.

5 SOME CRITICISMS OF KLINE

Kline's book is very long (more than 1,200 pages), and, at the same time, a masterpiece of compression. It therefore seems unreasonable to complain about omissions. All the same there is one major branch of mathematics which is scarcely mentioned at all, but whose importance does seem to call for at least one chapter if not two. This branch is probability theory.

Kline explicitly states in the Preface that he is going to devote little space to probability theory, and justifies his decision as follows (p. viii):

> . . . some developments in mathematics such as the theory of probability . . . which are important today, did not play major roles during the period covered and have accordingly received little attention.

But this justification seems scarcely valid. Kline covers the period from early beginnings to the 1930s, and during the last two and a half centuries of this time, probability theory can certainly lay claim to have played a major role in mathematics.

Admittedly probability theory was a latecomer on the mathematical scene. No start with the subject seems to have been made by the Egyptians, Babylonians, Greeks, Hindus or Arabs—a surprising fact since games of chance and gambling were well-known in these cultures. However from the time of the famous Pascal–Fermat correspondence (1654), probability theory has always been a topic of considerable interest to mathematicians.

The hundred years from the late seventeenth to the late eighteenth centuries saw the works of James Bernoulli, de Moivre and Laplace in which the first forms of the law of large numbers, and of the central limit theorem were proved. When treating this period of time, Kline devotes a chapter to Analytic and Differential Geometry in the Eighteenth Century (ch. 23), and one to the Calculus of Variations in the Eighteenth Century (ch. 24). Surely the importance of the developments in probability demands at least equal space.

The period from the early nineteenth century to the 1930s also saw remarkable advances in probability. First there is the mathematical work of Poisson, and then, later in the nineteenth century, the formation of the famous Russian school (Chebyshev, Markov and Lyapunov) who developed the study of the laws of large numbers, and the central limit theorem, as well as beginning the study of dependent events. This school continued in the early twentieth century with

Kolmogorov and Khintchine. A famous event was the publication in 1933 of Kolmogorov's axiomatisation of probability within a measure-theoretic framework.

Contemporary with the Russian school, who were mainly interested in mathematical probability, were the English school of biometricians (Galton, Karl Pearson, R. A. Fisher and 'Student') who were mainly interested in applied probability and statistics. To them are due most of the tests of significance and methods of estimation actually used by practising statisticians. Once again these nineteenth and early twentieth century developments do seem to deserve a chapter.

Not only are the results of probability theory of great mathematical interest, but many of them are also of great practical importance. Up till the middle of the nineteenth century the main fields of application of probability were insurance and the social sciences (*e.g.* Quetelet), though there were also some applications to astronomy. From the 1850s to the 1930s, however, probability penetrated the natural sciences as well: both Physics (statistical mechanics, radioactivity, Brownian motion and quantum theory), and Biology (genetics). These practical applications are an additional reason for devoting some attention to probability. In his earlier [1954] book, Kline has three very interesting chapters (chs. 22–24) on probability theory. It is therefore surprising that he does not devote more space to the topic in his longer history.

My next criticism concerns a peculiar value judgment of Kline's. He has a curious aversion to the theory of matrices and determinants. He writes (p. 795):

> Though determinants and matrices received a great deal of attention in the nineteenth century and thousands of papers were written on these subjects they do not constitute great innovations in mathematics.

This seems to me unfair. I would judge that a modern course on linear algebra contains as much interesting mathematical content as one on classical analysis, or topology, Moreover determinants and matrices are applicable to some extent in physics (quantum mechanics), and especially in economics—where the 'linear model' is a basic tool. (See, for example, Gale [1960].)

My last criticism concerns another omission of Kline's. One of the most remarkable mathematical developments of recent times was Robinson's discovery in 1960–1 that (in his own words, [1966], p. vii):

> . . . the concepts and methods of contemporary Mathematical Logic are capable of providing a suitable framework for the development of the Differential and Integral Calculus by means of infinitely small and infinitely large numbers.

Robinson's *Non-Standard Analysis* casts a new light on the history of calculus. Robinson himself in the last chapter of his book (*op. cit.* ch. 10, Concerning the History of the Calculus, pp. 260–82) attempts some historical reassessment, and he begins the chapter by pointing out the need for such reassessment (*op. cit.* p. 260):

> The history of a subject usually is written in the light of later developments. For over half a century now, accounts of the history of the Differential and Integral Calculus have been based on the belief that even though the idea

> of a number system containing infinitely small and infinitely large elements might be consistent, it is useless for the development of Mathematical Analysis. In consequence, there is in the writings of this period a noticeable contrast between the severity with which the ideas of Leibniz and his successors are treated and the leniency accorded to the lapses of the early proponents of the doctrine of limits.

Kline does not mention *Non-Standard Analysis*, and he tends to stick to the traditional view of the history of the calculus which Robinson here opposes. Thus, for example, Kline writes (p. 954):

> Even Poisson believed that there are positive numbers that are not zero, but which are smaller than any given number however small.

Perhaps in the light of non-standard analysis, a more sympathetic view of Poisson's position could be taken.

A good recent account of non-standard analysis is to be found in Bell and Machover [1977] chapter 11, pp. 531–75. Lakatos wrote an as yet unpublished paper extending Robinson's work on the reassessment of the history of calculus in the light of non-standard analysis. This will be in the second volume of Lakatos's *Philosophical Papers*—due to appear shortly.

DONALD GILLIES
Chelsea College, University of London

REFERENCES

Andrews, A. [1967]: *Greek Society*. Pelican Books.
Bell, J. L. and Machover, M. K. [1977]: *A Course in Mathematical Logic*. North-Holland.
Boyer, C. B. [1968]: *A History of Mathematics*. Wiley.
Gale, D. [1960]: *The Theory of Linear Economic Models*. McGraw-Hill.
Kline, M. [1954]: *Mathematics in Western Culture*. Allen and Unwin.
Kline, M. [1972]: *Mathematical Thought from Ancient to Modern Times*. Oxford University Press.
Kline, M. [1975]: 'Summary of "*The Break from Science*" ', *Historia Mathematica*, **3**, (1976), pp. 463–4.
Kline, M. [1976]: 'Carl B. Boyer—In Memoriam', *Historia Mathematica*, **3** (1976), pp. 387–94.
Lakatos, I. [1963–4]: 'Proofs and Refutations', *British Journal for the Philosophy of Science*, 14.
Robinson, A. [1966]: *Non-standard Analysis*. North-Holland.

SCIENTIFIC REALISM AND PERCEPTION*

1 In his recent book James W. Cornman makes a comprehensive attempt to analyse, reconstruct, and compare the most representative metaphysical theories of perception.[1] The book is very rich in information and crammed with specific argumentation. Broadly speaking the book is interesting from three points of

---

*Review of J. W. Cornman [1975]: *Perception, Common Sense, and Science*. Yale University Press. £11.00. Pp. vii+420.

[1] Throughout this paper I will refer to the mentioned book by phrases like 'Cornman's book' *etc.*, and the page references in such contexts thus of course are to this book.

view. First, it contains a detailed treatment of many central philosophical problems of perception. Secondly, there is a comprehensive discussion of scientific realism and instrumentalism as well as of the role theoretical terms and entities in scientific theorising, with special reference to the problems of perception. Thirdly, the book presents interesting historical analyses and reconstructions of Berkeley's, Locke's and Kant's views on perception. As this review article appears in a philosophy of science journal I have chosen to concentrate below on the second of the mentioned topics.

Cornman's book consists of an introduction and two main parts, the first on phenomenalism and the second on realism. The leading idea in the book is to discuss the central phenomenalist and realist theories of perception and to evaluate them in the light of the results of a detailed philosophical analysis and some general metaphilosophical principles of comparison.

Cornman starts his discussion by questioning the need for sensa (sense data), as opposed to (objectless) sensings in theories of perception. He concludes that there are neither strong empirical nor linguistic reasons for sensa.

After this prelude Cornman goes on to analyse phenomenalism. Here his emphasis is on Berkeley's phenomenalism, which he reconstructs as a kind of reductive theoretical phenomenalism. As Berkeleyan phenomenalism presupposes (or at least seems to presuppose) the correctness of scientific instrumentalism with respect to ordinary scientific theoretical terms, Cornman is led to a lengthy discussion of instrumentalism. The main conclusion of the first part of the book is that the most feasible phenomenalist theories are Berkeleyan phenomenalism, sensum instrumentalistic phenomenalism, and sensing instrumentalistic phenomenalism.

In the second part of his book Cornman concentrates on naive realism, Lockean, Kantian and Sellarsian indirect realism as well as on (compatible) common-sense realism. At the end Cornman thinks he is able to conclude that his compatible common-sense realism is the most adequate metaphysical theory of perception and the external world. Compatible common-sense realism combines a restricted version of minimal scientific realism with the view on perception according to which to perceive a physical object is to have it (minimally) perceptually presented (in a certain specific sense).

As to important new ideas Cornman's book does not contain very much. Its strength rather lies in its rich information content and in its argumentation. In general, Cornman attempts to present detailed arguments for his thesis. In the case of many problems he does indeed provide more detailed arguments than were previously available in the literature. On the other hand, as will be seen, not all of his central arguments are successful. Furthermore, there are some clear lacunae in his argumentation as well.

I would here like to make a remark concerning Cornman's method of philosophising. He seems to think that the best way to argue for a thesis is to present a sound *deductive* argument for it. (A tongue-in-cheek remark I feel tempted to add here is: the longer and the more complicated an argument is the better it is according to Cornman.) To be sure, this is a good method in many cases, but yet it is important to realise its limitations. First, there are nondeductive modes of argumentation. Secondly, in the case of the most fundamental, axiom-like philosophical theses the deductive mode does not work, in any case. Thirdly, in using this method of argumentation Cornman sometimes comes dangerously close (to say the least) to committing the following fallacy. He considers a certain thesis C (which he does not like) and somebody's argument for it. He reconstructs the argument as a deductive one. Then he argues that (at least) one of the premises is not acceptable and that the conclusion thus is not acceptable. Next he may go on to consider some other arguments for C and to make similar remarks about them. After all this he concludes that C is false. Now, of course, C is not here deductively concluded to be false. The only reasons given for its falsity are the mentioned failures to prove C true deductively, and the adequacy of this as an argument for the falsity of C is not really discussed (see pp. 201, 339, and Cornman's discussion of the theoretician's dilemma argument as examples).

Let me now proceed to a detailed critical discussion of some of the central claims of the book. As I said, the emphasis will be on issues in the philosophy of science.

2 In the first part of his book Cornman is concerned with defending Berkeleyan phenomenalism (and, to a much less extent, other instrumentalistic versions of phenomenalism). Berkeley seemed to think that pure scientific theoretical terms (comparable to our current terms 'electron' and 'neutrino') must be instrumentally construed as non-referring terms. On the other hand Berkeley thought that physical object terms, such as 'cherry', are referring terms (and, in Cornman's construal, observational theoretical terms), but what they refer to are bundles of sensa. Mental terms again are to be construed as theoretico-reportive terms in Berkeleyan phenomenalism, Cornman argues.

To what extent Cornman's interpretation and reconstruction of Berkeleyan phenomenalism is justified I will not discuss here. In any case, the claim that Berkeley was an instrumentalist regarding pure scientific theoretical terms is generally accepted, and that is enough for us below.

Let us now proceed to discuss the alleged correctness of scientific instrumentalism, which is so central for Cornman's book. Following Cornman, we shall use the *theoretician's dilemma argument* to give logical structure to our discussion. Cornman considers several, increasingly more refined, versions of this argument. I will present below this argument by using Cornman's numbering of the theses in it. (The second and third premises appear in formulations which make them congruent with the rest. Cornman does not explicitly formulate them.) The final version of the theoretician's dilemma argument for "linguistic" scientific instrumentalism can now be stated as follows:

(1) Theoretical terms either serve their purpose or they do not serve it.

(2*a*) If they do not serve it, then they are not needed for scientists to develop acceptable scientific theories, and neither theoretical terms nor variables are needed to provide acceptable scientific explanations and predictions of empirical data.

(3*a*) If they serve their purpose, then they establish epistemic systematisation among empirical statements.

(4*b*) If theoretical terms in a theory, *S*, establish epistemic systematisation, then either the Craigian transcription of *S* or the Ramseyan transcription of *S* preserves its epistemic systematisation.

(5*d*) If either Craigian transcriptions or Ramseyan transcriptions preserve epistemic systematisation, then no theoretical terms are needed for scientists to develop acceptable scientific theories, and neither theoretical terms nor variables are needed to provide acceptable scientific explanations and predictions of empirical data.

(6*a*) Therefore theoretical terms are not needed for scientists to develop acceptable scientific theories, and neither theoretical terms nor variables are needed to provide acceptable scientific explanations and predictions of empirical data.

Conclusion (6*a*) can be interpreted as saying that "linguistic" instrumentalism is acceptable. (6*a*) does not yet give us a sufficient reason for thinking that (i) theoretical terms are non-referring terms nor that (ii) there is no descriptive need for theoretical terms, nor for believing that (iii) there are no (scientific) theoretical entities.

Let us accordingly complete the theoretician's dilemma argument, using Cornman's penultimate version employing the third of the above non-equivalent conclusions (we will later take up Cornman's version):

(7*b*) If no theoretical terms are needed for scientists to develop acceptable scientific theories and neither theoretical terms nor variables are needed to provide acceptable scientific explanations and predictions of empirical data, then there is no reason to postulate the existence of any referents of theoretical terms, that is, any theoretical entities.

(8) If there is no reason to postulate the existence of any theoretical entities, then it is unreasonable to believe that there are any theoretical entities, that is, it is reasonable to accept scientific instrumentalism.

(9) Therefore, it is reasonable to accept scientific instrumentalism.

Conclusion (9), which follows from (6*a*), (7*b*) and (8), says, then, that it is reasonable to accept a rather strong, ontological kind of scientific instrumentalism. (Note, however, that this version does not yet claim that there are no theoretical entities.)

Before we go on to discuss scientific instrumentalism it should be noted that the fate of the theoretician's dilemma argument depends, or may depend, somewhat on how theoretical and observational (or, more broadly, empirical) terms are characterised. It seems that, for instance, Cornman's own explications of these notions are not problem-free (see Tuomela [1973], pp. 14–16 on that). However, I think that it is possible to give a viable *methodological* characterisation of theoreticity and observationality, in any case (see Tuomela [1973], pp. 17 ff. for such an attempt). If there happened to be several non-equivalent ways to make that distinction that would hardly affect our discussion below.

Let us first consider the argument (1)–(6*a*) for linguistic instrumentalism. Cornman accepts the argument, whereas I do not. In particular, I am willing to accept (1), (2*a*) and (3*a*), but I reject (4*b*), (5*d*) and the conclusion (6*a*). I will below present reasons for not accepting (4*b*) and (5*d*) on the basis of Cornman's arguments and, in addition, reasons for rejecting these two premises.

Let us start our detailed discussion with (4*b*). I will take it for granted that both the Craigian and the Ramseyan transcription of a theory preserves the deductive observational (empirical) systematisation achieved by the theory (see *e.g.* Tuomela [1973], chapters 2 and 3 for a relevant presentation and proofs of all this). The real trouble begins with inductive systematisation, for no one has so far been able to create a transcription method that can be proved to effectively preserve the inductive observational systematisation achieved by the non-transcribed theory. However, Cornman claims that his ***extended*** Craigian and Ramseyan transcription programmes do the trick. But here he is wrong. It is instructive to see why.

Cornman considers a theory S (employing theoretical predicates) and its Craigian transcription C–S and claims that the Craigian procedure can be extended to handle all ***epistemic*** systematisation (*viz.* both ***deductive and inductive*** systematisation) as follows (p. 161):

> First, we add to the deductive axioms and rules of L [the underlying logic of S] a set of axioms for the probability calculus, P, and a set of inductive rules, R (for Lehrer, RD1 and RI). Second, we claim that S achieves some epistemic systematization in just those cases where some $E_i \supset E_j$, which is not a theorem of L, P, or R, is derivable solely from S by L, P, and R. Third, for any statement of the form $E_i \supset E_j$, which is not a theorem of L, P or R, just those that are derivable from S by L, P, and R, that is, those I shall call the "***epistemic***" theorems of S, are to be taken as (deductive) theorems of C–S. Consequently, any statement, for example $E_i \supset I(O_2, b, O_1, b)$ is an ***epistemic*** theorem of S just in case it is a theorem of C–S that is deducible from an axiom of C–S consisting of the statement itself, in this case $E_i \supset I(O_2, b, O_1, b)$, conjoined the appropriate Gödel number of times. Of course, each set of axioms, comprising either S, L, P, or R, should be effectively defined and be distinct from each of the other three sets, and the corresponding proof procedures should also be effectively defined. But given that this can be done for L, I see no additional problems for doing it for P and R, especially for a set of inductive rules like Lehrer's rules.

In the above quotation the statement '$I(O_2b, O_1, b)$' is to be read 'O_2b is inducible from O_1b'. E_i, O_1b and O_2b are empirical sentences, where 'empirical' means, roughly, observational in a broad sense.

In order to facilitate our subsequent discussion we will redefine exactly the key notions appearing in the above quotation. We shall essentially follow Kaufman's [1974] corresponding set-theoretical characterisations, which have been endorsed by Cornman (p. 155).[1] Let us call Cornman's notion of inducibility I_1. Let $\mathbf{P}$ be the set of all the usual probability axioms and theorems. $\mathbf{I}$ is defined to be the set of axioms and theorems of inductive logic, where any set of axioms and rules governing the behaviour of I, is understood to constitute an inductive logic (*cf.* Kaufman [1974], pp. 217, 221). We define for formulas A, B:

(1) A is inducible$_1$ from B (*i.e.* $I_1(A, B)$) if and only if $\{B\} \cup \mathbf{P} \nvdash A$ and $\{B\} \cup \mathbf{P} \cup \mathbf{I} \vdash A$.

---

[1] In Niiniluoto [1972] a criticism was presented against Cornman's extended transcription procedure as it was formulated in Cornman [1972]. Kaufman [1974] replied to Niiniluoto's criticism. Cornman [1975] endorsingly comments on Kaufman's paper but, oddly enough, does not mention Niiniluoto's rejoinder to Kaufman (see Niiniluoto [1974]).

(2) A is inductively derivable from B if and only if $\{B\} \nvdash A$ and $\{B\} \cup \mathbf{P} \cup \mathbf{I} \vdash A$.

(3) A theory S achieves some inductive systematisation if and only if for some empirical formulas A and B, A is inductively derivable or inducible from $\{S\} \cup \{B\}$, and $\{B\} \cup \mathbf{P} \cup \mathbf{I} \nvdash A$.

(4) S achieves some epistemic systematisation if and only if it achieves some deductive or some inductive systematisation (*viz.* just in case $\{S\} \cup \{B\} \cup \mathbf{P} \cup \mathbf{I} \vdash A$ and $\{B\} \cup \mathbf{P} \cup \mathbf{I} \nvdash A$, for some empirical sentences A and B).

(5) A is an epistemic theorem of S if and only if A is an empirical sentence and $\{S\} \cup \mathbf{P} \cup \mathbf{I} \vdash A$ and $\mathbf{P} \cup \mathbf{I} \nvdash A$.

Kaufman [1974] shows that the extended Craigian procedure guarantees the preservation of epistemic systematisation if and only if the set $\mathbf{E}_S$ of epistemic theorems of S is functionally equivalent for epistemic systematisation to S, *i.e.* for any empirical sentences A and B such that $\{B\} \cup \mathbf{P} \cup \mathbf{I} \nvdash A$, A is an epistemic theorem of $\{S\} \cup \{B\}$ if and only if A is an epistemic theorem of $\mathbf{E}_S \cup \{B\}$.

Given the above explications we can proceed to some quick critical comments. (*Cf.* Niiniluoto [1974], who presents closely similar criticisms and some other ones as well.) The above notions of inducibility and inductive derivability of Cornman and Kaufman may seem odd as soon as we notice that they are defined in terms of the ordinary notion of deducibility, *i.e.* $\vdash$. Our suspicions soon become confirmed when we compare these notions with intuitively more satisfactory notions of inducibility and inductive derivability. Concentrating on inducibility, we may consider a simple probabilistic explication of inducibility in terms of positive probabilistic relevance:

(6) A is inducible$_2$ from B (*i.e.* $I_2(A, B)$) if and only if $p(A/B) > p(B)$.

The explicit definition of I_2 in terms of probabilistic relevance is not so central here; a partial definition might do as well. Let us here call such more liberal notions inducibility$_2$ notions, too. (Furthermore, there are other intuitive notions of inducibility, *e.g.* those related to a "high probability" criterion.)

On the basis of (1) we may now see that the following properties hold for I_1, and they are easily seen not to hold for *e.g.* I_2 (*cf.* Niiniluoto [1974], p. 226):

(7) If $I_1(A, B)$, then, for any C, either $\{B \,\&\, C\} \cup \mathbf{P} \vdash A$ or $I_1(A, B \,\&\, C)$.

(8) If $I_1(A, B)$ and $I_1(C, A)$, then $I_1(C, B)$ or $\{A\} \cup \mathbf{P} \vdash B$.

(9) $I_1(A, B \,\&\, C)$ if and only if $I_1(B \supset A, C)$.

(10) If B and $I_1(A, B)$, then A (given that the statements in $\mathbf{P}$ and $\mathbf{I}$ are true).

Here (7) says that we may add premises to inducibility$_1$ statements. (8) gives a transitivity property of I_1. (9) says that the inductive analogues of the deduction theorem and its converse hold for I_1. Finally, (10) states that, oddly enough, I_1 is necessarily truth-preserving. The failure of principles of the kind (7), (9) and (10) is often taken as essential to induction. It is therefore no surprise that none of the previously proposed notions of inducibility in the literature of the topic satisfy these principles.

The above remarks should suffice to show not only that inducibility$_1$ and inducibility$_2$ are completely different notions but that something quite essential is omitted from inductive systematisation if one decides to operate with inducibility$_1$. Essentially similar remarks as our above ones apply to Cornman's and Kaufman's notion of inductive derivability, which we may call inductive$_1$ derivability. In other words, inductive$_1$ derivability and inductive$_2$ derivability are different, and inductive$_1$ derivability fails to capture the core of our presystematic idea of inductive derivability. (Notice that in the case of non-probabilistic statements inductive$_1$ derivability coincides with inducibility$_1$, for in that case $\{B\} \nvdash A$ if and only if $\{B\} \cup \mathbf{P} \nvdash A$.)

Moreover, also the corresponding notions of inductive$_1$ systematisation and inductive$_2$ systematisation differ analogously (see Niiniluoto [1974], p. 228 for a discussion of this).

In Niiniluoto and Tuomela [1973] it is shown that there are simple theories which employ theoretical predicates and lack non-tautological empirical (observational) deductive theorems and which achieve some empirical inductive$_2$ systematisation. This systematisation is something that need not be preserved by Cornman's extended Craigian transcription programme. Notice, furthermore, that a theory achieving inductive$_2$ systematisation need not have any inductive$_1$ nor even inductive$_2$ theorems, the set of which is what the extended Craigian procedure is meant to axiomatise.

We have thus seen that Cornman's extended programme does not guarantee the preservation of inductive$_2$ systematisation (and hence not of epistemic$_2$ systematisation). What is more, it need not, after all, even always preserve inductive$_1$ (and epistemic$_1$) systematisation either, and this is of course a major internal fault in the programme. For, simply, the notion of proof in the context of deriving epistemic$_1$ theorems should be *effective* (recursive), and this is of course the basic thing to show when constructing a transcription procedure. Cornman himself notices the requirement of effective proof procedure in the paragraph quoted earlier, but he does nothing whatsoever about it. In any case, this requirement entails that the set E_S of epistemic$_1$ theorems must be *recursively enumerable*. But this is not in general the case. For example Lehrer's rules of inducibility have in fact been shown to be non-effective, and this also applies to their versions *RD*1 and *RI* discussed by Cornman (*cf.* Niiniluoto [1972]). Cornman again states in the quoted passage that he sees no specific problem in doing what has been proved to be impossible! Indeed, it must be said as a conclusion that presently there seems to be very little hope for creating an effective extended Craigian replacement programme for any viable set of inductive rules.

Our basic critical points against Cornman's extended Craigian transcription procedure apply, *mutatis mutandis*, to his extended Ramseyan transcription procedure as well. As, furthermore, Cornman says next to nothing about the whole procedure (except for a superficial one-sentence characterisation on p. 169), there is no need here to discuss it in detail. The hard fact is that Cornman has not shown that there is an effective extended Ramseyan replacement programme to preserve epistemic systematisation any more than he has proved a Craigian one to exist.

There are some additional minor flaws in Cornman's discussion of extended replacement programmes but in view of the gravity of our above criticisms there is no need to discuss them here.

We have in effect shown above that Cornman has failed in his attempt to make premise (*4b*) of the theoretician's dilemma argument acceptable. What is much more, the mentioned positive results of Niiniluoto and Tuomela [1973] concerning the indispensability of theoretical terms within inductive systematisation give a strong argument for the falsity of (*4b*) (and of (*6a*) as well).

Let us now proceed to a discussion of premise (*5d*) of the theoretician's dilemma argument. Cornman discusses an objection to it due to Sellars but dismisses it. But there he is wrong. Let us go into some details of this Sellarsian objection, which claims that scientific generalisations require theoretical terms for their explanation.

According to Sellars,

> microtheories not only explain why observational constructs obey inductive generalizations, they explain what, as far as the observational framework is concerned, is a random component in their behaviour, and in the last analysis it is by doing the latter that microtheories establish their character as indispensable elements of scientific explanation and as knowledge about what really exists (Sellars [1963], p. 122).

Why is Cornman wrong in claiming that Sellars is wrong here? First, Cornman has misunderstood what Sellars means by a 'random component' above. Cornman thinks that this is connected to the value of the probability measure in statistical generalisations so that if $p(O_2/O_1) = \frac{n}{m}$, then theoretical explanation is needed in the Sellarsian sense only when $\frac{n}{m} < \frac{1}{2}$. But this is wrong. Sellars means by 'randomness' something like the instability and approximative character of observational (empirical) laws, and this is not connected to the value of $\frac{n}{m}$. (In the case of the inductive-probabilistic explanation of *singular* empirical statements it may, however, be reasonable to impose the Leibniz-condition, *viz*. that the inductive probability of the explanandum relative to the explanans be over $\frac{1}{2}$.)

In any case, the Sellarsian objection to (*5d*) can be taken to involve two claims: (1) empirical generalisations require theoretical explanation, and (2) the explanation of at least some singular empirical statements requires theoretical explanatory concepts. Let us examine these claims by means of the following example due to Sellars and discussed by Cornman (although these authors do not explicitly use probabilistic terminology). Suppose we are given the following three empirical generalisations:

$$p(O_2/O_1) = \frac{n}{m} \qquad \text{(G1)}$$

$$p(O_3/O_1) = 1 - \frac{n}{m} \qquad \text{(G2)}$$

$$(x)(O_2(x) \supset \sim O_3(x)). \qquad \text{(G3)}$$

A theory, S, designed to explain G1 and G2 is given by the conjunction of the following axioms:

$$p(T_1/O_1) = \frac{n}{m} \tag{A1}$$

$$p(T_2/O_1) = 1 - \frac{n}{m} \tag{A2}$$

$$(x)(O_1(x) \mathbin{\&} T_1(x) \supset O_2(x)) \tag{A3}$$

$$(x)(O_1(x) \mathbin{\&} T_2(x) \supset O_3(x)). \tag{A4}$$

Now let $O_1(x) = x$ is a piece of gold, $O_2(x) = x$ is something that dissolves in *aqua regia* at rate p, $O_3(x) = x$ is something that dissolves in *aqua regia* at rate q, $T_1(x) = x$ is something with microstructure r, and $T_2(x) = x$ is something with microstructure s. Now Sellars's claims are that (1) theory S and hence its theoretical terms are needed to explain G1 and that (2) they are also needed to explain that something a which is O_1 is also O_2. Hence, G1 is explained by deducing it from S, with dependence on its theoretical terms. That a which is O_1 is also O_2 is explained by S and the fact that a is T_1 rather than the more usual T_2. Clearly, $T_1(a)$ is essential to this explanation.

However, Cornman claims that "if a system explains randomness by having the generalization that expresses the randomness as an epistemic theorem, then the system transcribed in either [*i.e.* Craigian or Ramseyan] way also explains it" (p. 177). But this is flatly wrong. For instance, in the case of deductive systematisation explanation cannot of course be identified with deduction. Consider *e.g.* the explanation of G1. Cornman claims that *C–S* explains it because it has as its axiom the conjunction consisting of G1 conjoined with itself a certain number of times. But that represents a clear case of self-explanation, which cannot of course qualify as a proper explanation. I have discussed this particular problem in Tuomela [1973], chapter VII, where I also show that such an "explanation" does not satisfy the *DE*-model of explanation presented in that book. In that same context I also discussed the explanation of empirical generalisations such as G1 by means of the Ramsey sentence of a theory. I argued that the (deductive) explanation given in terms of the original theory is, in any case, better than the Ramseyan explanation already because in its case a stronger explanans is employed. In all, then, transcribed theories do not successfully compete with the original ones in explaining observational generalisations.

Let us next consider singular explanation. Is $T_1(a)$ after all essential to the explanation of why a, which is O_1, is also O_2? Let us first consider Ramseyan explanation. First we notice that $T_1(a)$ is an element of our explanans. We may therefore, at least for the sake of argument, grant that $T_1(a)$ be included as a conjunct under the scope of the existential quantifier quantifying over the predicate T_1. Given this much, one may claim, and Cornman does indeed claim, that the Ramsified explanans preserves the explanatory power of the original explanans (including $T_1(a)$ as its element). But here again (just as in the previous case) the Ramsified explanans is weaker in explanatory power than the original explanans. Furthermore, we must be able to use the variable ϕ_1 replacing T_1 as a "quasi-constant" (*e.g.* in making reports such as $\phi_1(a)$). As I argued in Tuomela [1973], this practically amounts to realistic (as opposed to instrumentalistic) theorising.

How about the Craigian case then? As the Craigian transcription of the original explanans gives us as the relevant empirical theorem only $O_1(a) \supset O_2(a)$

conjoined with itself a number of times, but no law, our instrumentalist seems to be in deep trouble. However, Cornman's remedy now is to use the generalisation $(x)((x = a) \supset (O_1x \supset O_2(x))$ for our explanation. But this won't do, because certainly this generalisation cannot be a law of nature (its scope is *a priori* limited to one individual only and it is thus equivalent to the singular statement $O_1(a) \supset O_2(a)$), and an explanation requires (implicitly or explicitly) a law, I take it.

I conclude that Cornman has not succeeded in rebutting the Sellarsian objections. Hence we also have reasons to reject (5*d*). As a matter of fact there are many other reasons, over and above these Sellarsian ones, to reject (5*d*). For instance, considerations related to the observational growth of a theory, to the economy of theorising, to the non-eliminability of theoretical terms in the semantical Ramseyan sense, and so on, can be cited against (5*d*). These "gains" due to theoretical concepts have been discussed in detail in Tuomela [1973]; see especially chapter VII. I will not here review them.

We have argued that premises (4*b*) and 5*d*) of the theoreticians dilemma argument are false. Therefore we have reasons for claiming instrumentalism (at least in its linguistic form) to be false (or unacceptable). This has some consequences concerning Cornman's evaluation of various theories of perception. For, obviously, we now have an argument against all the instrumentalistic versions of phenomenalism, such as Berkeleyan phenomenalism, sensing instrumentalism and sensum instrumentalism.

In chapter 5 Cornman ends his treatment of theoretical phenomenalism by discussing some final objections to it. As he construes Berkeleyan phenomenalism as *theoretical*, as opposed to analytical, phenomenalism (*i.e.* physical objects are taken to be theoretical rather than logical constructions of sense data or ideas), certain standard objections against phenomenalism, raised by *e.g.* Ayer and Chisholm, do not apply here. There is one major argument against theoretical phenomenalism, due to Sellars [1963], which yet has to be discussed. The argument is, or at least seems to be, roughly that theoretical phenomenalism requires the adoption of the so called standard account of theories. However, the argument goes, as the standard account (which entails instrumentalism) is incorrect, so is theoretical phenomenalism. The standard account is wrong in general (because of its positivistic and instrumentalistic character), Sellars would claim. But it is also wrong (when applied to this particular case) for the additional reason that it requires the existence of independently confirmable sensum generalisations. As there are no such generalisations, the argument goes, we cannot even start applying the standard account. (What Sellars's argument really is may be debated; the above is my suggestion.)

I will not here go into the details of Cornman's lengthy argumentation against Sellars. I am willing to accept Sellars's general argument as sketched above but not the more detailed version of it reconstructed by Cornman. The presentation of all the details here would take too much space. Let me just point out that Cornman's argumentation in part relies on his above kind of instrumentalistic views on explanation, which we argued to be wrong. Thus, of course, I don't here either accept that the Ramsey sentence of a probabilistic law preserves the explanatory power of the original law. To go to the example discussed on pages 199–200 (also see p. 196), assume that the law $p(O_4(x)/O(x) \,\&\, T_3(x)) >$

0.70, together with $O_1(b)$ and $T_3(b)$ explains why b has O_4. Now, as indicated, in my view the Ramsified explanans $(E\phi)$ $[p(O_4(x)/O_1(x))$ & $\phi(x)) > 0.70$ & $O(b)]$ does not fare as well.

How about the Craigian transcription then? Cornman claims that "the Craigian procedure shows how to achieve a set of observation postulates from which $p(O_4(b), O(b)) > 0.70$ is derivable as a theorem" (p. 200). But this is wrong. There is no way to derive $p(O_4(b), O_1(b)) > 0.70$ (whatever such a singular probability statement is taken to mean) from $p(O_4(x)/O_1(x)$ & $T_3(x)) > 0.70$ and $T_3(b)$ by means of the calculus of probability. Cornman is wrong in rejecting Sellars's claim on the basis of the above kind of considerations related to explanation (they are his basic point against Sellars's argument, see p. 200). Similarly, Cornman's above error also falsifies his claim on page 201 that his argument has shown not only the standard account of theories but also the Sellarsian account of them to be incorrent.[1]

When discussing Berkeleyan phenomenalism Cornman claims that physical object terms (*e.g.* 'cherry') may be needed for the explanation of sensings. (Recall that Cornman construes physical object terms as observational ***theoretical*** terms.) Now I am a little puzzled about this, for it would seem that Cornman's instrumentalistic eliminations in terms of extended Craigian or Ramseyan procedures could be applied (supposing them to work, contrary to fact) to physical object terms as well. I think that Cornman owes us an argument concerning why he thinks the mentioned transcription procedures apply only to the pure theoretical terms of science but not to such theoretical terms as 'cherry' as well.

Otherwise I find Cornman's reconstructions of Berkeleyan phenomenalism interesting and illuminating quite independently of whether they are historically adequate or not.

3 The second part of Cornman's book is concerned with ***realist*** theories of perception. He begins by a discussion of naive realism, and in spite of his overall sympathies, he takes the well known time-gap argument to refute this doctrine. Next he discusses ***indirect*** realisms, primarily the Kantian, Lockean and Sellarsian versions of it. I agree more or less with Cornman's interesting analyses of indirect realism and I will not here go into any details. One related thing I would like to mention is that in my view direct Sellarsian realism (*i.e.* Sellarsian realism without sensa) still escapes Cornman's criticisms against indirect Sellarsian realism. Cornman later claims that this doctrine becomes refuted

---

[1] The mentioned misinterpretations and mistakes of Cornman may have been the main reason for his ending up with the following "correct" account of theories (n. 14, p. 201):

(1) Some confirmable, open [*i.e.* not containing indexicals such as 'I'] observation generalisations are neither universal nor probabilistic with $n/m > \frac{1}{2}$;

(2) all explainable, singular observation statements are explainable without theoretical terms; and

(3) some explainable, singular observation statements are not explainable by open generalisations that have no predicate variables and no theoretical constants.

I find (1) true, but uninteresting. (2) is false, given our above arguments. I am puzzled by (3). If we are to understand it so that in some cases generalisations employing theoretical constants may be needed for explanation, then (2) is falsified. In any case, no matter what sense we make of (3), the conjunction of (1), (2) and (3), even if it were true, gives us a pretty meagre "account of theories".

because it presupposes "moderate scientific realism", which Cornman thinks (incorrectly, in my view, but consistently with his own approach) to be unacceptable.

The position on perception Cornman adopts is (minimal) common-sense realism. Although I will not really discuss this view in detail below let me here concisely summarise this position by means of two theses presented by Cornman on pages 284–5:

(MCR1) The world consists in large part of perceivable physical objects that are not perceivers, that exist unperceived, and that are affected either by being perceived or, generally, by changes in the usual conditions in which they are perceived; many of these objects have and are perceived to have at least the minimally required sensible properties, that is, the observable occurrent properties of size, shape, weight, solidity, texture, motion (rest), location, sensuous colour, and hotness (coldness).

(MCR2) The event of someone, *s*, perceiving a physical object, *p*, at a time, *t*, is identical with the event of *p* being minimally perceptually presented to *s* at *t*, that is:

(1) *s* immediately perceives *p* at *t*, *and*

(2) for any minimally required sensible physical property, *Q*, *if*, at *t*, *s* should be a normal observer who immediately perceives *p* in conditions optimal for distinguishing whether or not *p* has *Q*, and in so doing he has an experience of *p* as having *Q*, *then*, at *t*, he would immediately perceive a property that *p* has approximately at *t*, and that is either *Q* or some property which is indistinguishable from *Q* by unaided normal human perceivers, even in optimal conditions.

As can be seen from the second thesis, this common-sense realism is not refuted by the time-gap argument in spite of the close similarities between this doctrine and naive realism.

Cornman next conjoins common-sense realism with minimal scientific realism, a metaphysical doctrine summarised by the following thesis (pp. 266–7):

(MSR) Each nonsentient physical object and each sentient being has (at least) the constituents, properties, and relations ascribed to it by the theoretical scientific terms that are required for the best (nontranscribed) scientific explanations of its perceivable behaviour and effects.

The conjunction of minimal common-sense realism and minimal scientific realism is called ***compatible (minimal) common-sense realism***. But is it after all possible to consistently conjoin minimal scientific realism and common-sense realism? We are here invited to consider Eddington's two (but possibly identical) tables—the perceptible one and the microphysical one. Compatible common-sense realism states that any table has properties "quite like" those we under normal conditions take it to have and also the properties that are ascribed to it in scientific theories. This Cornman calls the "identity thesis", because it in effect states that what Eddington called two tables are really the same table.

More generally, the identity thesis can be formulated as follows (p. 287):

(IT) The conjunction consisting of the following statements is true: the statement of minimal common-sense realism, the statement of minimal scientific realism, and the hypothetical statement that ascribes to physical objects the properties and constituents they have on the condition that minimal scientific realism is correct.

I understand Cornman to think that the hypothetical statement is to be evaluated by reference to current scientific knowledge (at least this is how this statement should be taken).

In the latter part of his book Cornman thus wants to be a (minimal) scientific realist. This sounds rather odd in view of his strong instrumentalist sympathies in the first part of his book. It would seem that one cannot, for conceptual reasons, consistently be both a scientific realist and a scientific instrumentalist. However, Cornman claims that he is able to neutralise the theoretician's dilemma argument so as to make his position consistent. We shall soon see how far he succeeds in this schizophrenic task which resembles the squaring of the circle.

Cornman starts his qualified defence of minimal scientific realism (and the identity thesis) by discussing some objections to realism derived from current science. His discussion of quantum physics leads him to think that at best a restricted minimal scientific realism can be accepted. Thus he sees no possibility for a realistic interpretation of the state vector term 'ψ'. But given an instrumentalistic interpretation of 'ψ' he is yet willing to accept a realistic interpretation of elementary "particle" terms.

One may question Cornman's conclusions here. For one thing, there seem to be realist moves to make against the above conclusions even on the basis of our current knowledge. One such move is simply to put the problems about the wave-particle interpretation into the compartment of quantum measurement theory and to claim that quantum theory itself says nothing about either particles or waves. This makes it possible to interpret 'ψ' as representing a physical state of an individual (be this individual called a quanton or whatever) which obeys the axioms of the theory. Secondly, Cornman anyhow seems to take very seriously current scientific evidence. He takes it to support not only an instrumentalist interpretation of 'ψ' but the fact that at best restricted minimal scientific realism is a viable version of scientific realism. I do not think we in any case yet have obtained a reason to give up scientific realism (in any sense) for *all* branches of science because of the situation in quantum physics. Furthermore, one may try to emphasise the regulative (or normative) aspect of a thesis like MSR and not to take it as a purely descriptive thesis as Cornman seems.

How does Cornman then defend his scientific realism? As this matter is important for the evaluation of Cornman's position we have to go into some details here. Cornman considers the following conclusion from premises (5*d*) and (7*d*) of the theoretician's dilemma argument:

(10) If either Craigian or Ramseyan transcriptionism preserves the epistemic systematisation of theories, then there is no reason to postulate the existence of theoretical entities.

Cornman's basic argument now is that even if scientific theoretical entities may not be needed (and are not needed in his view) for adequate scientific explanation (*cf.* (5*d*)), yet an ideal complete explanation of a phenomenon may require realism with respect to at least some of the terms that best explain the phenomenon. This claim is backed by claiming that a complete (causal) understanding requires realism in the mentioned sense and that complete explanation requires complete (causal) understanding.

I am quite happy with all this. But now Cornman goes on to discuss whether the closest that we can approach ideal complete explanation requires referents for certain best-explaining theoretical terms of science. He suggests that a realist attempting to explain events in terms of constituent causes seems to approach quite closely the ideal of complete explanation (*cf.* (5*d*)).

Now there seem to be two approaches available to an instrumentalist, Cornman argues: "either to eschew any attempt to understand an event in terms of its causes or constituents, or to postulate causes and constituents other than those implied by a realistic construal of scientific explanations" (p. 320). Cornman quickly dismisses the first possibility. He then discusses neo-Malebranchean occasionalist explanations in terms of non-constituent causes, *viz.* God's willings, and claims that they give as good explanations as are obtainable within a realist schema (p. 322–3):

> Consider, for example, two different causal explanations of why when the temperature of a fixed volume of gas increases a certain amount, the gas pressure increases a corresponding amount. A scientific realist would postulate that the gas consists of molecules whose mean kinetic energy increases an amount that is a function of the amount of increase in temperature. This causes a corresponding increase in the average velocity of the molecules, which causes an increase in the average force with which the gas molecules hit the walls of what contains the gas. Consequently, the gas pressure increases a certain amount. Our neo-occasionalist, however, need only postulate that when the temperature increases a certain amount there is just one 'intervening' occurrence: God causes the gas pressure to increase an amount that is a certain function of the temperature rise.

Cornman goes on to say that he can find no reason to prefer either the occasionalist or the scientific realist explanatory schema. I find this quite remarkable and could hardly disagree more. How is Cornman's occasionalist going to individuate different God's willings empirically, for example? What does the occasionalist's explanatory theory exactly look like, and what are its empirically testable consequences? I just cannot see how it could compete with the kinetic theory at all.

After all this Cornman, however, concludes that he has found *some* reason to use the realist explanatory schema. Thus he claims that (10) should be rejected. Furthermore (10) is to be rejected *because* (7*b*) is false. (He continues to think that (5*d*) is acceptable.) His conclusion thus is that there is some reason to postulate theoretical entities even if no theoretical terms are needed for acceptable *scientific* theories, explanations and predictions. Indeed, he is willing to claim even that there is some reason for postulating *scientific* theoretical entities even if no scientific theoretical terms are needed for *ideal* scientific explanation. According to Cornman it is reasonable to think that ideal explanations require more than ideal scientific explanations and to hold that, nevertheless, scientific theoretical entities come as close as any postulated entities to meeting this additional requirement (whatever it more exactly is).

Now I do not, first of all, accept that no scientific theoretical terms are needed for acceptable scientific theories, explanations, and predictions (see above for my reasons for rejecting (5*d*)). Secondly, I do not see any good reason to distinguish between ideal explanation and ideal scientific explanation. (What is that "additional requirement"?) Thirdly, I don't understand what the *scientific* entities spoken of in connection with non-scientific ideal explanations really are or could be. Fourthly, I see no reason to reject (7*b*), provided we understand it to speak about *ideal* (or, perhaps, ideally acceptable) scientific theories, explanations and predictions rather than merely acceptable ones. If we thus interpret (7*b*) in this way I think Cornman's arguments in this connection do not suffice for its rejection. Given this, it follows that he is committed both to linguistic and to ontological instrumentalism (*i.e.* to 9), as far as his argumentation is concerned. Thus I think that his attempt to reconcile (a weak version of) scientific instrumentalism with (a weak version of) scientific realism has not succeeded.

Cornman's own attempted way out of the problem is to reject (7*b*), as said, and to substitute for it the following more complicated thesis (p. 324):

(7*c*) If scientific theoretical terms are necessary to develop acceptable scientific theories and to provide acceptable scientific explanations and predictions of empirical data, and the identity hypothesis is implausible, then it is unreasonable to believe that there are any scientific theoretical entities.

(In Cornman's own version the last word is'terms' rather than 'entities'. I believe that to be a mistake.)

I am willing to regard (7*c*) as true, especially if our newly interpreted (7*b*) is taken to be "a part" of (7*c*). It is only that I do not see good reasons to complicate the original theoretician's dilemma argument by using the statement about the implausibility of the identity hypothesis in the antecedent of (7*c*). (Naturally we also need another premise asserting that the identity hypothesis is implausible before we get the original conclusion (9).)

The identity hypothesis simply is irrelevant to the original theoretician's dilemma argument. In fact, the truth of this hypothesis by itself entails the falsity of instrumentalism, for the identity hypothesis contains the thesis of minimal scientific realism as one of its conjuncts. (Similarly, the falsity of the identity hypothesis due to the falsity of minimal scientific realism by itself entails instrumentalism.)

Is the identity hypothesis acceptable? Consider again Eddington's two tables. A scientific realist would presumably say that what "*really*" exists is the scientific table—a certain "cloud" of elementary "particles" or quantons or something of the sort. The perceptible table again does not really exist (not at least in the strong sense the scientific table does).

Let us for the moment assume that some reasonably clear sense can be made of the Sellarsian notions of the scientific image and the manifest image (*cf.* Sellars [1963]). The concept of a perceptible table belongs to the conceptual framework of the manifest image and the concept of a scientific table is again something conceptualised by science (*i.e.* the scientific image). Now, allowing the relativisation of truth to a conceptual framework, we may say that it is a truth in this framework, and indeed a categorial truth, that the table (here: a single logical subject) is coloured, solid, has a certain texture, and so on. These properties are not, however, truly ascribable to the scientific table (an aggregate of logical subjects). Furthermore, what is truly ascribable to the table within the framework of the scientific image is "really real" or "more real" than what is truly ascribable to the table within the manifest image—*scientia mensura.*

Given that the above realist picture of the situation is acceptable, we are really dealing with two concepts of table and, accordingly, "two different tables", we may say. There are indeed going to be two different tables in a strong ontic sense if we accept that there are no bare substratum-tables, *viz.* that the scientific tables and the perceptible table must be regarded as ontically structured (by means of some constitutive properties or something like that) and of course differently structured in the appropriate ways.

The above gives a general line of a realist argument against the identity hypothesis. Cornman, however, only takes up some specific arguments against this hypothesis. Thus he considers two arguments Aune and Sellars have presented. The first states that if an object is sensuously coloured and has constituents, then at least some of its basic constituents are coloured, but if (minimal) scientific realism is correct, then none of the basic constituents of physical objects are coloured. Thus, contrary to minimal common-sense realism, no physical objects are coloured if minimal scientific realism is true. However, after reconstruing the argument in more detail Cornman claims that it has two weak premises, which suffice to make the argument unacceptable to a compatibilist.

I can not here go into all details but I am afraid Cornman has misunderstood the argument. Let us thus quote the discussed passage from Aune (quoted from pp. 329–30 of Cornman's book):

> Suppose that we have taken the molecular theory of matter fully to heart and have merged it with our ordinary notions regarding apples and billiard balls as aggregates of exotic particles. Suppose also that we continue to speak of these familiar objects as colored. The question then arises: can we consistently use our color words in their ordinary sense?

Now Cornman, in his reconstruction, thinks that Aune assumes that if an object is coloured then at least some of its constituents must be coloured. This is an incorrect interpretation of the quoted passage, it seems. What it does (implicitly) claim is that if an object conceived of within the manifest image (*e.g.* as an apple) is coloured (*e.g.* is red) then also the same object conceived of within the scientific image as an aggregate of elementary particles also is coloured. The argument says nothing about whether individual elementary particles are coloured or not. Thus Cornman does not really discuss Aune's argument in its proper sense.

The second argument against the identity hypothesis Cornman considers is this. The colours that a common-sense realist claims cover some surfaces of certain observable physical objects are continuous expanses of colour, and this clearly requires that physical objects be spatially continuous. But this spatial continuity is incompatible with one consequence of a minimally realistic interpretation of science, namely, that the basic constituents of observable physical objects are spatially discrete particles. Cornman rebuts this argument. (I will not here present the details.)

After discussing the above two arguments against the identity hypothesis Cornman thinks he is warranted in claiming that compatibilism escapes unscathed. What is more, he believes that there are no other reasons for rejecting compatibilism (p. 339). But this is too quick a conclusion. As we saw, Aune's first argument was not properly countered by Cornman, and there may, in addition, be other convincing arguments against the identity hypothesis. In summary, in view of the importance of the truth of the identity hypothesis, Cornman gives too few reasons for accepting it. (In fact, he does not really present any direct detailed arguments in favour of it at all, only arguments against accepting some arguments against it.)

Cornman's conclusion now is that, since he thinks the identity hypothesis can be accepted as true, the theoretician's dilemma argument (with (7*c*)) does not go through, and that compatible (minimal) common-sense realism is acceptable.

The final task for Cornman now is to compare compatible common-sense realism with those theories of perception that he thinks have not yet been refuted. He rejects realist theories based on stronger doctrines than *minimal* scientific realism, his main reason here being that using them would make the identity hypothesis so strong as to be unacceptable. Thus he now thinks he can give a final refutation of *e.g.* both Sellarsian and Lockean indirect realism as well as Sellarsian direct realism. But as we just saw above the identity hypothesis is very problematic. Furthermore I think that even stronger forms of scientific realism than the minimal one can be consistently conjoined with suitably formulated common-sense views on perception, for instance, as is done in Sellarsian realism. In other words, it seems that a suitable "identity assumption" can be formulated with moderate or Sellarsian realism as one of its conjuncts.

Cornman correctly rejects Kantian nondirect realism on the grounds that, according to scientific realism, science can be thought to uncover precise information about things-in-themselves (now, of course, construed as scientific objects).

The realist theories of perception that are still left to compete with compatible minimal common-sense realism, given that Cornman is taken to have been successful in his above arguments, are "incompatible" common-sense realism (which accepts instrumentalism and rejects the identity hypothesis), compatible sensing realism, and compatible sensum realism. We recall from our discussion of the first part of the book that also some versions of phenomenalism, *viz.* Berkeleyan reductive phenomenalism and two versions of eliminative phenomenalism (sensum instrumentalistic phenomenalism and sensing instrumentalistic phenomenalism), still stand unrefuted in Cornman's opinion. (Note: given that our earlier arguments against instrumentalism are correct these doctrines would not even get off the ground.)

For reasons that space does not allow me to discuss here Cornman concludes (and I think correctly) that there is more reason not to accept sensa than there is to accept them. This eliminates Berkeleyan phenomenalism, sensum instrumentalistic phenomenalism and compatible sensum realism. Finally, relying in addition on his meta-philosophical principle P3, Cornman claims that compatible common-sense realism conclusively fares better than naive realism, direct and Kantian realism, as well as incompatible common-sense realism, and finally, compatible sensing realism. (Principle P3 says that a philosophical theory, *T*, which is consistent with more pretheoretical commonly accepted beliefs of mankind than another theory, *T′*, is to be preferred.) I will not here go into the details of this final elimination process mainly because we have already found some of Cornman's premises and thesis needed in this elimination to be either false or shaky. Even Cornman himself regards his final conclusion to prefer compatible minimal common-sense realism as tentative and provisional.

As my conclusion let me then say that Cornman's acceptance of compatible common-sense realism as the best metaphysical theory of perception and the external world is not as strongly argued for as could be desired. I also have some doubts about the whole task as I find it too huge to be reasonably treated in one volume. Furthermore, even given such a fully satisfactory treatment philosophers would be likely to disagree on the principles of evaluation (and their weighing), in any case. The main importance of Cornman's book lies in the great amount of information and detailed argumentation it contains. Although some of his arguments are mistaken and some beside the point, the book nevertheless contains lots of insightful and good argumentation on difficult philosophical problems in the best tradition of analytical philosophy. This makes the book rewarding and useful.

RAIMO TUOMELA
University of Helsinki

REFERENCES

Cornman, J. W. [1972]: 'Craig's Theorem, Ramsey-Sentences, and Scientific Instrumentalism', *Synthese*, **25**, pp. 82–128.

Kaufman, S. A. [1974]: 'The Preservation of Epistemic Systematization within the Extended Craigian Program', *Synthese*, **28**, pp. 215–21.

Niiniluoto, I. [1972]: 'Inductive Systematization: Definition and a Critical Survey', *Synthese*, **25**, pp. 25–81.

Niiniluoto, I. [1974]: 'Inducibility and Epistemic Systematization: Rejoinder to Kaufman', *Synthese*, **28**, pp. 223–32.

Niiniluoto, I. and Tuomela, R. [1973]: *Theoretical Concepts and Hypothetico-Inductive Inference, Synthese Library*, Dordrecht and Boston: Reidel.

Sellars, W. [1963]: *Science, Perception, and Reality*. London: Routledge and Kegan Paul.

Tuomela, R. [1973]: *Theoretical Concepts. Library of Exact philosophy*. Vienna and New York: Springer.

Reviews

GELLNER, E. [1974]: *The Legitimation of Belief.* Cambridge: Cambridge University Press. £4.25. Pp. 210.

The core of Ernest Gellner's position in this book consists of three main contentions. The first is that epistemology, if not the Queen of the sciences, is at least their House of Lords or their Supreme Court, 'the final court of appeal in intellectual dispute' (p. 27). So he enjoins us: 'examine how you know things, the criteria you have for assessing the validity, the legitimacy of cognitive claims. If those criteria themselves be sound and legitimate, they will transmit their own validity to the substantive knowledge which has been authenticated by them; but if not, then the whole edifice is worthless' (p. 28). Epistemology is an evaluative, not a descriptive enquiry. What it provides us, or ought to provide us, with is a 'cognitive ethics' (pp. 41, 183). It does so by a radical and Cartesian beginning: the terms of reference permissible for the epistemologist 'require that the thinker allows for the dissolution of his world, and makes only tentative, ironic, interim use of anything drawn from it. Epistemology tries to excogitate convincing norms under such terms of reference. How do we find out which world we are in, presupposing none?' (p. 43).

The second central contention is that this intellectual court of appeal ought to be governed in its decisions by two and only two principles: 'A claim to knowledge is legitimate only if it can be justified in terms of experience' and 'A claim to knowledge is legitimate only if it is a specification of a publicly reproducible structure'. The first principle is elucidated in terms of an empiricism of which Locke and Russell are taken to have been the chief protagonists. The importance of the appeal to experience is held to be that it allows no system of beliefs to be judged in its own case: 'experience asserts its sovereignty . . . by proscribing the construction of circular, self-maintaining worlds. It achieves this by its sketch of a world composed entirely of atomic data, independent of each other and of all theories, subservient to none' (p. 112). This principle Gellner nicknames that of "The Ghost", borrowing but transforming Ryle's use of that metaphor. What Gellner preserves from Ryle's accusatory use is the thought that every experience is someone's experience, unsharable by anyone else. It is in virtue of this thought that there is a tension between Gellner's first principle and his second principle.

The first formulation of Gellner's second principle was, as I have already noted, in terms of the public reproducibility of tests of beliefs. But Gellner immediately makes it clear that the second principle 'does insist on a certain kind of *explanation*; that which is invoked in the explanation must be some kind of a machine' (p. 63). It is in terms of the model of a machine that 'human and other intelligent behaviour' is best explained (p. 85). Hence while the first principle leads towards 'subjectivism and the vision of a discontinuous, fragmentary world, of "bundles", "sensations" and so forth', the second 'naturally leads to the assumption of a permanent, continuous reality, quite independent of consciousness . . .' (p. 73). But although Gellner's two principles of epistemological selection often may seem to point us in opposite directions, the tension is lessened when we recognise that each has a different function. The first principle is a selector of ideas and information 'of great merit', but is misleading if used as a model of behaviour; the second is a selector of explanations, which is also of some use in selecting ideas and information (p. 108).

Gellner's third central contention amounts to an interpretation of history. History falls into two and only two great periods. On the one side of the great divide is what Gellner variously describes as belief in 'The world of regular, morally neutral, magically unmanipulable fact' (p. 180), 'The attainment of a rational, non-magical, non-enchanted world' (p. 182), 'modernity' (p. 152), and 'modern industrial civilisation' (p. 149). The institutions and practices of natural science are of course the central feature of modernity. Modern societies are contrasted with 'traditional' or 'primitive' societies in which 'cognition, the discovery or the endorsement of beliefs is an event . . . in the social and moral world. Hence they are subject to the same kinds of obligations and sanctions as are other kinds of conduct' (p. 166). Because of this relationship of the moral to the cognitive crucial elements in their belief-systems are exempted from rational criticism.

Gellner argues that all other discontinuities and transitions in human history are relatively unimportant: 'it is unlikely that all illiterate tribal societies are similar in cognitive power, and downright absurd to say the same of all pre-scientific civilisations. Nevertheless, to grade them is a fairly pointless exercise'. The great culture-hero of this transition is Kant; its sociologist is Max Weber. What we have to learn from Weber is in part that we are irredeemably "modern". Gellner makes this point by asserting that 'such on-going traditions as this world contains have long been torn asunder by the extraordinary forces which constitute the modern world. They are poor things in any case and no modern population would consent to live within their walls' (p. 203). So Gellner closes his book by adding to his version of empiricism and his version of mechanism two other elements in what he calls the 'emerging consensus' (p. 206). One is what he calls 'ironic cultural nationalism'—an 'acceptance of "forms of life", from styles of food, handshakes and wallpaper to political rituals or personal relationships' which is 'ironic, tentative, optional and above all, discontinuous with serious knowledge and real conviction' (p. 207). The other is a concern with the development of industrial civilisation, with sociology. Gellner pictures our civilisation as having endured a shipwreck, presumably that of traditional forms of belief, and as having now constructed a raft whose planks are these four elements. 'These are the four usable planks of our raft. If there are any others, they have entirely escaped my attention' (p. 208).

This then is Gellner's positive case. I have necessarily simplified it by abbreviating it. In particular I have omitted Gellner's abusive polemics against views which he takes to be rivals of his own. The targets are mostly familiar ones to readers of Gellner's earlier writings—Oakeshott, Hegel, early Wittgenstein and late Wittgenstein, although George Lichtheim, William James and Quine have now joined this distinguished roll-call. There are also respectful, less polemical discussions of certain theses advanced by Chomsky and by Popper. If I largely ignore this aspect of the book it is because to do otherwise might distract us from considering the merits or demerits of Gellner's own position.

It is also necessary to avoid being distracted initially by Gellner's style of philosophising, although later I shall suggest that the style may be importantly related to the substance. Gellner's style intersperses pages of serious exposition with jokes, abuse, sneers. Among chapter and section headings we find 'Get lost', 'Hanging Judges', 'Let a hundred flowers bloom' and 'Instant God'. It is as if parts of the book were written by a philosophical Archie Rice, unsustained by John Osborne's talent. But important philosophy in the past has sometimes been written in aesthetically unpleasing prose. It would be frivolous to be distracted by Gellner's unhappy style from examining his central contentions. Nonetheless there is an initial barrier to such an examination and it lies in Gellner's lack of precision about his own enterprise.

Gellner writes that he is concerned with 'the criteria you have for assessing the validity, the legitimacy of cognitive claims' (p. 28) and at an early stage he equates these with 'the criteria of truth and knowledge' (p. 29). We might therefore expect discussion of the nature of truth and of knowledge and also, since Gellner favours 'cognitive exploration', of what the growth of knowledge consists in. But our expectations are disappointed. We get initial brief, even cursory, glances at the verification principle which is praised at one stage (p. 33) and damned at another (p. 110), at the importance of falsifiability (p. 110) and at a number of topics in the history of science. None of this provides much insight into what Gellner believes about 'criteria of truth and knowledge'. It is clear however, that, whatever else Gellner is arguing for, he is committed to one major epistemological claim: 'that experience asserts its sovereignty . . . by proscribing the construction of circular, self-maintaining worlds. It achieves this by its sketch of a world composed entirely of atomic data, independent of each other and of all theories, subservient to none' (p. 117). This claim as Gellner presents it, encounters three types of difficulty.

The first is that it is true if and only if rival theories of scientific importance can indeed always be tested against genuinely neutral data which provide an important court of appeal. Gellner cites Thomas Kuhn to the contrary (p. 79) and he could have cited N. R. Hanson and Paul Feyerabend, both of whom present stronger arguments than does Kuhn. Yet Gellner, while rejecting Kuhn's view, nowhere answers it.

The second type of difficulty also arises from recent episodes in the philosophy of science. In Sir Karl Popper's own work and in the successive work of J. W. N. Watkins, Joseph Agassi and Imre Lakatos the continuous reformulation of the problem of testability—let alone the debates with Carnap and with Kuhn—have made it clear that we do not yet possess any entirely satisfactory account of how we are to understand the evaluation of competing theories. Gellner himself expresses a certain scepticism: 'I rather doubt whether anything resembling a formal criterion, outside and independent of the theories that are being tested . . . could be found or devised . . .' (p. 184). Then he adds 'but we can leave this problem to the philosophy of science. Our concern is with how the arena come to be set up at all, and not with the rules which govern its internal procedures'. But one who sneers so readily at the lack of argument in others must not shrug off quite so cavalierly the lack of argument in his own case. For in part what is at issue in these debates is precisely whether natural science as a form of knowledge can be distinguished from other types of belief in the way that

Gellner's position requires. More particularly, if we cannot find, as Gellner suspects we cannot, 'anything resembling a formal criterion, outside and independent of the theories that are being tested' what becomes of Gellner's central thesis? The virtue of empiricism allegedly was that it required theories to be judged by *independent* criteria yielding independent data.

Thirdly Gellner in advancing his central thesis appears to embrace an empiricism which does not entail all of its familiar consequences, but he never tells us how. Classical empiricism engenders what many of us take to be insoluble problems about personal identity, other minds, causality and induction. Presumably an empiricist has some obligation therefore—an obligation accepted willingly by Quine, for example—to explain why empiricism can be detached from these difficulties. But since Gellner appears not to notice, let alone accept, this obligation it remains quite unclear what the content of his empiricism is.

In all three areas which I have identified there is a curious lack of arguments by Gellner in favour of his own positions. When there is argument, it is not only brief, but almost always directed *against* some other position and by no means always against those positions which Gellner's own thesis requires him to rebut. Hence the book is a curious conjunction of negative polemic and unsupported positive assertions. This is as true of those parts of it which concern topics conventionally allocated to moral philosophy or to history as it is to those concerned with epistemology. Gellner appears for example to believe that emotivism is the theory central to modern moral philosophy—he may, in some sense, be right, but once again argument is lacking—and he thinks that as a matter of historical fact it was work by philosophers on topics in epistemology which 'could and did deter magic'. In fact a central part of his historical message is that modernity has earned our gratitude by expelling magic; yet elsewhere he notes the coexistence in the culture of California cities of the most advanced science and technology with rampant promagical antirationalism. But once again he shrugs off an objection rather than argues: 'it seems to me to constitute only a partial and superficial aspect of' the contemporary scene (p. 192).

Finally there is a difficulty of another kind. On Gellner's account of legitimate belief, how do philosophical beliefs fare? What in particular is the status of Gellner's own thesis? Gellner attacks all those who try to render their beliefs irrefutable; but he also argues both that reductionism 'is a luminously true and certain idea' and that 'The hope that it could ever be denied or refuted is absurd' (p. 107). Thus there are apparently beliefs held by Gellner himself which are exempt from the criteria invoked against other by Gellner; but Gellner never explains or justifies this.

It would be all too easy at this point to be simply dismissive. A book composed of jokes, sneers and unsubstantiated assertions may appear not to need or deserve a serious critique. Yet if we consider this book as a symptom of a philosophical condition rather than as an apologia for it—and this would be unforgivable unless we had first considered its merits as argument—we shall find that this book displays an astonishing if embarrassed sensitivity to what is happening in philosophy today. Jokes and sneers are often symptoms of status anxiety and ambivalence; and status anxiety and ambivalence are highly appropriate psychological states for anyone deeply identified with the central theories of the Anglo-American philosophical tradition.

About the central theories of that tradition Gellner is clearly in the right; its main self-set task was that of epistemological justification. But the task was defined in terms that ensured its ultimate defeat and this in at least three ways. First there is a deep incompatibility between any form of belief in philosophical empiricism and belief in the natural sciences. For central to empiricism is a fabricated notion of experience as essentially individual and private; while central to the natural sciences is a notion of experiment as public and repeatable. The import of science is that we ought to distrust experience in general; it is only certain selected types of public experience, interpreted in certain canonical ways, from which we can hope to learn. By contrast empiricism in its rake's progress from its seventeenth-century vocabulary of ideas to its twentieth-century vocabulary of sense-data invents a universe of increasingly irrelevant private objects.

Secondly the whole model of justification implicit in the enterprise was at fault. Epistemology cannot provide a deductively ordered account of the foundations of science, for the whole metaphor of foundations was a mistake. The first prerequisite for carrying through the epistemological programme was, and was rightly perceived by both rationalists and empiricists to be, the refutation of scepticism. Yet scepticism never was in fact refuted; and certain types of circularity and incommensurability seem ineliminable in those argumentative structures which are central to our scientific beliefs about nature.

The philosopher who first clearly identified this break-down of the epistemological project was Hegel—and it is perhaps not surprising that Gellner treats Hegel with some hostility. But Hegel, with his deep insights into what Kant has to teach, supplied not only what was the first and is still the best diagnosis of the break-down of the epistemological project; he also provided an alternative approach. Theories, beliefs, research programmes, perspectives are not to be justified in any absolute Cartesian or Lockean way, for that is an impossible, originally Platonic project. What can be required however is that they should receive such justification as they can *vis-à-vis* their competitors in each particular historical context in which they occur. Theories, beliefs, research programmes, perspectives of any age come to us bearing the marks of their defeats and victories in such encounters. What was always hidden in Popper's falsifiability criterion was the requirement that no theory, belief, research programme or perspective should be exempted or allowed to exempt itself from such encounters. It is not as Gellner falsely supposes that they can or cannot be 'justified in terms of experience'; it is rather that they can or cannot be vindicated against their historically encountered rivals, part of the rivalry consisting in alternative competing accounts of how the evidence of experience is to be construed.

With the recognition of these three lessons that Hegel has to teach there ought to go also the recognition that what has been the dominant tradition in Anglo-American philosophy—for empiricism and rationalism have retained their hold even in analytical disguise and even when there have been attempts to give a Kant cut down to analytical size his due—is now doomed to a barren repetition of its earlier failures. Gellner's use of his metaphor of the shipwreck and the raft turns out to be inappropriate; what he takes to be a raft sank some time ago.

Gellner's reassertion of these doomed positions is thus a touching act of faith in a disintegrating tradition. The fact that he offers us almost no arguments for his faith should be taken as a sign of his integrity; even he apparently recognises that there *are* no good arguments for his position. His apparently blatant inconsistency is no more to be deplored than that of any dying martyr who thanks God for his goodness. Gellner's jokes then are not as irrelevant as they at first appear. They are most charitably and properly to be understood as the would-be cheerful, pardonably self-indulgent monologue of a gravely wounded soldier from a defeated army who still protests the rightness of his cause long after the battle has been lost. How sad!

ALASDAIR MACINTYRE
Boston University

AYALA, F. J. and DOBZHANSKY, T. (*eds.*) [1974]: *Studies in the Philosophy of Biology, Reduction and Related Problems*. London: Macmillan.

This book is the product of a conference on "Problems of Reduction in Biology", organised by the editors and held at the Villa Serbelloni, Bellagio, Italy in September 1972. It includes nineteen position papers written in advance of the conference by nineteen of the twenty participants (internationally prominent biologists and philosophers, including Peter Medawar, Jacques Monod and Sir Karl Popper). Although these position papers were circulated before the conference to facilitate discussion, only the last discussion session is published (short comments are appended to some of the papers). Jacques Monod took part in the last session of the conference, and the book concludes with the discussion which followed his brief remarks concerning *Chance and Necessity* (Alfred A. Knopf: New York, 1971).

The editors do not provide a convincing rationale for including all twenty papers in a single volume. They sought to convene a "fresh panel of participants", to avoid tiresome recapitulation of other recent symposia on reductionism (they allude to those organised by Waddington and Koestler). But few of these papers deal directly with reductionism and the best dispute the significance of that theme. F. Ayala's introduction leaves the reader unprepared for what is to come.

Jacques Monod refused to defend reductionism as other participants had expected, saying instead, "I didn't defend reductionism (in *Chance and Necessity*) because I do not believe in reductionism; as a matter of fact I do not know what it is" (p. 371). In a note added after the conference, Sir Karl Popper stated,

> In the text of this paper I have disregarded—perhaps carelessly, or because I dislike terminological minutiae—the distinction that can well be made between explanation in general, and reduction in the sense of an explanation by way of an established or more 'fundamental' theory. (p. 260 n.)

In a postscript, Popper added, "I should now be disinclined to call an explanation with the help of a new theory a 'reduction' " (p. 283). Since almost all the historical examples in his paper deal with cases in which a newly developed *explanans* was proposed for a more familiar but problematic *explanandum*, it is clear that Popper's paper deals with "explanation and the incompleteness of science" rather than with reduction. Morton Beckner and June Goodfield also expressed reservations:

> The important thing is not so much the truth about various reductionist theses, but insight into the conditions and strategies of the application of one science to another. These questions about the applicability of a lower to a higher-level theory are very broad; reduction is a special case. But for historical reasons, the special case has commanded an undue share of explicit attention. (Beckner, p. 175).

> I am overpowered by a feeling of *déjà vu* verging at times on the very edge of intellectual impotence. 'Reductionism; 'anti-reductionism'; 'beyond reductionism'; 'holism'. . . . The issue is a very old one recurring in various forms with unfailing regularity throughout biological history, and the feeling of impotence arises because, after all this time, the issue never seems to get any clearer. (Goodfield, p. 65)

The editors do little to overcome such feelings. Their papers do not address the substantive differences among the major contributors; they provide no summary or concluding chapter. The result is a book whose contributors talk past each other. The tasks of critical synthesis are left for the reader. The editors might well have been less hospitable, limited the contributions to one-half the present number, and provoked a really full discussion among the authors of the chosen papers.

The philosophical value of the book would have been much greater, for example, if the editors had arranged a more sustained and critical discussion of the papers by Karl Popper and Morton Beckner. Beckner's paper is a formal analysis of the concepts of hierarchy, of "event reduction" and of "strong organicism". Although he accepts Nagel's analysis of theory reduction, he finds "self-conscious seeking of theory reduction a rarity in the history of science" (p. 176). A more frequently employed strategy is the "revision of higher-level theory in a manner that facilitates event reduction", *e.g.* the application of organic chemistry to a particular biological problem such as allergic reactions. Popper, on the other hand, explicitly rejects the attempt to analyse the meaning of 'reduction', "I am opposed to the method of meaning analysis and to the attempt to solve serious problems by definitions" (p. 259). He turns instead to the discussion of examples of successful and unsuccessful reductions (he uses the term throughout his paper in the fashion rejected by his postscript). His paper deals with a wide range of historical cases, including the effort to "reduce" geometry and irrational numbers to natural numbers, the successive efforts to reduce Faraday's electromagnetic field to a Newtonian mechanism and then to reduce mechanics to electromagnetic theory, the effort to reduce chemistry to quantum mechanics via a theory of cosmology or cosmic evolution, and ending with the problem of "mind–brain liaison".

Popper's use of reduction in this paper has no connection with the concepts of hierarchy or theoretical level. Beckner's does. Popper uses the term in the sense of 'explanatory simplification'; a plurality of explanatory elements is a symptom of unsuccessful reduction:

> We have at lest four very different and still irreducible kinds of forces in physics: gravitation, weak decay interaction, electromagnetic forces and nuclear forces. (p. 266)

The basic argument of the paper is that while no scientific strategy is more informative than reduction (or the effort to achieve a simpler, more unified and more comprehensive theory), there are no correspondingly good arguments for "philosophical reductionism". The latter is suspect because of its link with the essentialist goal of ultimate explanation (the attempt to explain or reduce things by an appeal to something that is neither in need of, nor capable of, further explanation). Popper rejects what he calls an "anti-reductionist research programme", but insists upon the distinction of three "worlds" (of physical objects; of conscious and subconscious experience; of spoken language, social institutions and cultural products). This terminology is used to "emphasize the limited *autonomy* of these regions" (emphasis added, p. 275), and to clarify an argument for a "form of psychophysical interactionism":

> The thesis (is) that the physical world 1 is not causally closed, but open to the world 2 of mental states and events; a somewhat unattractive thesis for the physicist, but I think one that is supported by the fact that world 3 (including its autonomous regions) acts upon world 1 via world 2. (pp. 275–6)

Popper's position is close to that of John Eccles, whose "Cerebral Activity and Consciousness" discusses the results of surgical division of the corpus callosum of the human brain. These "split-brain" investigations have falsified the hypotheses of psychoneural identity and psychoneural parallelism, according to Eccles. This leads to speculation concerning "an incessant interplay at a level of interaction between World 2 and the liaison brain that is beyond our present level of understanding" (p. 103). Eccles espouses the "neurophysiological hypothesis that . . . the 'will' modifies the spatiotemporal activity of the neuronal network by exerting spatiotemporal 'fields of influence' that become effective through this unique detector function of the active cerebral cortex" (p. 101).

Beckner calls the position that "some higher level processes are autonomous with respect to lower levels" (p. 171) "strong organicism", and he argues that its main thesis entails vitalism. For Beckner, higher level processes in hierarchical systems are autonomous with respect to processes at some lower level if and only if the laws of the higher level processes are not fully determined by laws of the lower level. Such autonomy cannot be inferred from the irreducibility of theories at the two levels since this may reflect no more than a difference in conceptual structure or in the descriptive vocabularies of the two theories. The strong organicist, in order to support the view that some higher-level processes are autonomous with respect to lower levels, must show that there is *no* description of the events explained by the higher level theory under which those same events could be explained by the lower level theory.

The absence of an explicit exchange between Beckner and Popper leaves a troubling residue of doubt as to whether Beckner would categorise Popper's position on mind–brain liaison as "strongly organicist" or "vitalist". Popper does not attempt to show that *no* description of world 2 events is compatible with their explanation by the neurophysiological laws of world 1. On the other hand, both Popper and Eccles clearly state that events in world 2 and world 3 are capable of exerting an autonomous influence upon events in world 1. For Beckner, such claims must assume some "higher level" entity which suspends the laws of world 1 and then relaxes such suspension, *i.e.* such claims entail vitalism. However, Beckner considers only cases where the various levels or worlds are strata in a hierarchy. The relationships between world 1 and world 2, and between world 2 and world 3, as these are described by Popper, cannot be described as hierarchical without begging the basic questions at issue.

June Goodfield's contribution begins in the pessimistic fashion alluded to above ("the issue never gets any clearer"), but she looks for clarity in concrete historical detail rather than in abstract analysis. She discusses a number of nineteenth and twentieth-century biologists, primarily physiologists, but concentrates upon Claude Bernard, Paul Weiss and Jèrome Lettvin. She asks in what ways their position in the reductionist/anti-reductionist spectrum may have influenced their scientific practice. She argues that anti-reductionism makes a difference:

> Men like Bernard or Weiss . . . force biologists to turn their attention to certain phenomena which otherwise might be ignored, and see them as posing genuine problems. . . . Reductionist biologists have been more concerned with biochemical substances and biological structures, rather than interactions and the special conditions under which structures operate. (p. 81)

Weiss has an engineer's perspective on the two-way relationship of organism and environment which explains not only the organism's behavioural response, but also organic structure and form. The key idea is traced to Bernard:

> What Bernard did was to force us to focus on rather different kinds of questions: to look not so much at structures as at regulatory mechanisms, to study not only the physicochemical nature of the physiological process, but the special conditions under which it exists and because of which it has a quite special character. (p. 72)

Goodfield gives up her position quickly in the ensuing discussion, admitting (to Peter Medawar and Gerald Edelman) that:

> Problems as biological problems eventually will force themselves on to the attention of a scientist irrespective of the general philosophy with which he approaches the subject. When it arrives it may be only a question of 'ripe' or 'unripe' time. (p. 85)

and (to Sir Karl Popper) that

> Since reductionism (is) essentially a problem of a 'world-view' we should not expect it to have direct consequences within the laboratory, nor at the level of a theory, where the continuation of a theory is dependent on laboratory findings. (p. 85)

A more interesting discussion might have taken place if Goodfield had stood firm on her historical case studies, using them to challenge the generality or even the accuracy of the positions of Medawar, Edelman and Popper. She had argued that, because of the work of Bernard and Weiss, organismic concepts and principles ('integrated system of macromolecular structures and functions', 'open system', 'hierarchy of organisation', 'the only source of biological order is biological order') had become the common property of all molecular biologists. This argument ought to stand on its merits as an historical interpretation, but the discussion did not address those merits.

However, Goodfield also fails to discuss the possible difference between scientific practice on the one hand, and meta-methodological commitments (*e.g.* anti-reductionism, reductionism) on the other. She emphasises that Bernard and Weiss were hard headed experimentalists, not given to dogmatisms about *a priori* principles, and she draws all her information concerning their meta-methodological commitments from their actual scientific practice. This leaves us unenlightened as to the nature and extent of other possible influences in the formation of their views.

Dudley Shapere's account of the relations between "compositional" and "evolutionary" theories addressed the demarcation question directly. Shapere considers it appropriate to distinguish "those sorts of inadequacies or circumstances which lead to rejection of theories and those which do not". But he holds that the distinction between science and non-science is a matter of degree, not a sharp categorical distinction marked by the criterion of falsifiability. He makes the interesting point that a compositional theory (the chromosome theory of the gene, and the theory of population genetics based on it) was used to remedy an inadequacy in Darwin's evolutionary theory, an inadequacy which had not led to the rejection of Darwinism in spite of the fact that its significance was generally appreciated. The book contains no record of any discussion between Popper and Shapere on this point.

Shapere holds that the common philosophical analysis of 'reduction' is excessively stringent. So stringent that, on the one hand, 'reduction' is equivalent to 'destruction' (the complete elimination of the reduced field), and on the other hand, that there is no real case of reduction in the history of science. He seeks a conception "truer to the facts of science and its history". He regards the following list of conditions as sufficient for the legitimation of a search for a compositional theory: the domain of the theory exhibits a discrete, periodic order which is extensive, detailed, and precise; the domain is related to other domains (and with them forms a larger domain) in which compositional theories have been successfully sought in the past. Evolutionary theories may be sought if an ordered domain can be viewed as deriving its order from the increase or decrease of some relevant factor; and the reasonableness of this search for an evolutionary theory is increased if the domain's sequential ordering is temporal, and if it is temporally directed. Shapere reminds us that evolutionary (Darwinian) and compositional (Mendelian) theories may be unified without reduction.

The discussion following Shapere's paper leaves a number of important questions unanswered. We are told nothing about the processes which might lead to the unification of two compositional (or two evolutionary) theories, or about cases where one of two compositional (evolutionary) theories *replaced* the other. Nor were any questions raised about the processes by which consensus on the legitimating conditions for particular types of theory-search has actually been attained by different scientific communities in different historical periods. Perhaps Shapere meant to construct a single set of sufficient conditions, relevant for all historical circumstances. He describes the conditions in lean terms, free of non-scientific or metaphysical overtones. He never argues, however, that purely internal factors have been sufficient for the legitimation of the search for compositional or evolutionary theories throughout the history of science.

The book's only comprehensive record of a conference discussion was of that following Jacques Monod's brief comments. Unfortunately, that discussion was also inconclusive. Monod ignored Dobzhansky's cogent argument that his use of the expressions "pure" or "absolute chance" to describe organic mutations was unnecessarily misleading, since mutations occur in the context of existing biological organisation, including the organisation of the genome resulting from the action of natural selection in the evolutionary past. Nor did Monod's efforts to clarify the principle of objectivity satisfy his critics. First, he claimed that principle was related to Popper's criterion of demarcation and precluded statements or theories interpreting a phenomenon in terms of final causes on the grounds that such statements or theories cannot be proved wrong (p. 358). Since Monod also insists upon the objectivity of functional accounts of organic processes (using such terms as 'homeostasis', 'self-organization' and 'teleonomy'), Shapere pushed for clarification of the sense of final causality excluded by the principle of objectivity. Monod's reply was that the principle excludes animist systems which merge and confuse the notions of logical (or metaphysical) necessity with those of ethical obligation.

The book ends abruptly with Karl Popper's succinct but emphatic rejection of Monod's concession (to Donald Campbell) that religious or political superstitions might be worthy of respect, on functionalist grounds, if they could be translated into language which would make it plain that they are not competitive with science. Dobzhansky's paper had included a section on "evolutionary transcendence", dealing with the implications of man's self-awareness and his awareness of his own death. But the issues of religion and politics are not discussed elsewhere in the book. As a result, the book ends on a note for which the reader has not been prepared and which does nothing to conclude what has gone before.

EDWARD MANIER
University of Notre Dame

AUSTRALASIAN ASSOCIATION FOR LOGIC

The 1977 annual conference of the Australasian Association for Logic will be held at the University of Melbourne, Parkville, Victoria, Australia, from Friday 21 October, to Sunday 23 October 1977. Information about the conference, or about the A.A.L. in general, can be obtained from the Secretary, Ms. Jan Galle, c/o Philosophy Department, University of Melbourne, Parkville, Victoria, Australia 3052.

BRITISH SOCIETY FOR THE PHILOSOPHY OF SCIENCE

PROGRAMME OF MEETINGS 1977–1978

1977

10 October

Dr A. J. Leggett—The Quantum Measurement Problem

7 November

Dorothy Edgington—Probability, Non-Standard Logic and Conditionals

5 December

Professor Pavel Tichý—Truthlikeness and Its Enemies

1978

16 January

Professor Stanislav Andreski—Meaning and Validation in the Social Sciences

13 February

Dr G. C. Webster—The Problem of Biological Form

13 March

4.15 p.m. ANNUAL GENERAL MEETING

5.15 p.m. Presidential Address: L. Jonathan Cohen—Holism, Chess and Artificial Intelligence

24 April

Nils Roll-Hansen—Mechanism and Anti-mechanism: Some Suggestions for a Classification of Biological Methodologies

22 May

G. M. K. Hunt—Subjectivist Entropy

19 June

Professor Saul Kripke—[To be confirmed]

22–24 September

Annual Conference at the University of East Anglia

1979

21–23 September

Annual Conference at the University of Bristol

Meetings are held in the Haldane Room, University College, London, beginning at 5.15 p.m. unless otherwise indicated. Tea is served from 4.45 p.m.

GRAZER PHILOSOPHISCHE STUDIEN

INTERNATIONALE ZEITSCHRIFT FÜR ANALYTISCHE PHILOSOPHIE

GEMEINSAM MIT — *TOGETHER WITH*

K. ACHAM, R. M. CHISHOLM, R. FREUNDLICH, E. TOPITSCH & O. WEINBERGER

HERAUSGEGEBEN VON — *EDITED BY*

RUDOLF HALLER

Board of Consulting Editors: H. ALBERT (Mannheim, BRD), D. FØLLESDAL (Oslo, Norway), P. GOCHET (Liège, Belgium), J. HINTIKKA (Helsinki, Finland), J. KIM (Ann Arbor, Mich., USA), F. v. KUTSCHERA (Regensburg, BRD), K. LAMBERT (Irvine, Calif., USA), J. K. MACKIE (Oxford, England), G. PATZIG (Göttingen, BRD), M. PRZELECKI (Warszawa, Poland), W. STEGMÜLLER (München, BRD), P. WEINGARTNER (Salzburg, Austria)

Review Editor: Wolfgang L. GOMBOCZ

GRAZER PHILOSOPHISCHE STUDIEN is published twice a year by Editions RODOPI (Keizersgracht 302—304, Amsterdam, The Netherlands). Subscription per volume-institutions Dfl. 40,—/DM 40,—; individuals Dfl. 30,—/DM 30,—. A major objective of the journal is to stimulate and to strenghten communications between American and European analytical philosophy. Manuscripts to be considered for publication should be addressed to Rudolf HALLER, Philosophisches Institut, Universität Graz, A-8010 GRAZ, Austria. Further information and a copy of the „Guidance for Contributors to the GRAZER PHILOSOPHISCHE STUDIEN" will be sent upon request.

GRAZER PHILOSOPHISCHE STUDIEN

BAND 2 — 1976 — VOLUME 2

Aufsätze — *Articles*

Theorienstruktur in der Literaturwissenschaft . . . . Heide GÖTTNER
Interpretation und Zielsetzung. Betrachtungen zum Problem der Eigenart der juristischen Interpretation . . . . Ota WEINBERGER
Knowledge and Belief; Facts and Propositions . . . . Joseph MARGOLIS
Knowledge, Belief, Existence, and Quantifiers — A Note on Hintikka . . . . Wolfgang LENZEN
Über Wahrnehmung von Aspekten . . . . Petra von MORSTEIN
The Factuality of Facts . . . . Reinhardt GROSSMANN
On the Phenomenology of Partial Judgment . . . . John M. VICKERS
From Purity to Reference . . . . Ronald SCALES
Ad Baculum . . . . John WOODS & Douglas WALTON
Das normative Fundament der Sprache: Ja und Aber . . . . Eike von SAVIGNY
Handlungsgraphen. Graphentheoretische Modelle in der analytischen Handlungsphilosophie . . . Hans LENK
Laws in the Social Sciences . . . . V. Warren BOURGEOIS
‚Intentionale Erklärungen'. Kritische Gedanken zu Henrik von Wrights Sicht der Erklärung . . . . Reinhold BICHLER

Besprechungen — *Review Articles*

Hubert SCHLEICHERT: *Logischer Empirismus — der Wiener Kreis,* München: Fink 1975 . . . . Reinhard KAMITZ
Ernst TOPITSCH: *Die Voraussetzungen der Transzendentalphilosophie.* Kant in weltanschauungsanalytischer Beleuchtung, Hamburg: Hoffmann und Campe 1975 . . . . Stephan KÖRNER
Edo PIVČEVIĆ (Hrsg.): *Phenomenology and Philosophical Understanding,* London: Cambridge University Press 1975 . . . . Lothar ELEY
Alvin PLANTINGA: *The Nature of Necessity,* London: Oxford University Press 1974 . . . . John B. BACON
Karl ACHAM: *Analytische Geschichtsphilosophie.* Eine kritische Einführung, Freiburg: Alber 1974 . . . . Herbert SCHNÄDELBACH

Herausgeber — *Editor*

Prof. Rudolf HALLER, Philosophisches Institut, Universität Graz, Heinrichstraße 26, A-8010, Graz, Österreich/Austria.

Volume LXXXVI MIND—JANUARY 1978 Number 341

Edited by D. W. HAMLYN

Articles:

The Truth-conditions of Counterfactual Conditional Sentences	D. GOLDSTICK
Concepts of Evidence	P. ACHINSTEIN
Absolute Certainty	R. W. MILLER
Fakes	C. RADFORD
Pains, Brain States and Scientific Identities	S. WILLIAMS

Discussions:

Liberalism and Nozick's 'Minimal State'	G. SAMPSON
Using People	N. RICHARDS
Gettier's Notion of Justification	C. LOWY
Nozick on Appropriation	H. STEINER
Logic for Morons	R. L. V. HALE

Critical Notice:

Hegel by Charles Taylor	A. MANSER

Book Reviews

Published for the MIND ASSOCIATION by BASIL BLACKWELL

Orders to: Journals Department, Basil Blackwell & Mott Ltd.,
108 Cowley Road, Oxford OX4 1JF, England

Annual Subscription:
Institutions—£8.00 or $16.00
Individuals—£6.00 or $12.00
Single issues—£2.25 or $4.50

---

the review of

metaphysics

a philosophical quarterly

SEPTEMBER 1977 | VOL. XXXI, No. 1 | ISSUE No. 121 | $5.00

articles

ROBERT SOKOLOWSKI	Picturing
RICHARD J. BERNSTEIN	Why Hegel Now?
MERRIE BERGMANN	Logic and Sortal Incorrectness
JOSEPH L. BLAU	Emerson's Transcendentalist Individualism as a Social Philosophy

critical study

MAURICE A. FINOCCHIARO	The Uses of History in the Interpretation of Science

discussion

M. J. INWOOD	Solomon, Hegel, and Truth

philosophical abstracts *doctoral dissertations* *visiting philosophers from abroad*
north american philosophers abroad *professors emeriti* *announcements*

Individual Subscriptions $12.00 Institutional Subscription $18.00 Student Subscriptions $8.00

Jude P. Dougherty, Editor
The Catholic University of America, Washington, D. C. 20064
Twenty-Year Cumulative Index, 1947–1967 is available for $3.00 a copy.

dialectica

International Quarterly Review of Philosophy of Knowledge

Board of Directors (Switzerland)
P. Bernays, Bodmerstrasse 11, 8002 Zurich
Ed. Bertholet, case postale 68, 1880 Bex
F. Bonsack, 155, av. de Cour, 1007 Lausanne
J.-B. Grize, 15, La Traversiere, 2013 Colombier
D. Jakubec, Devin 98, 1012 Lausanne
H. Lauener, Längfeldstrasse 8, 3063 Ittigen/Berne
J.-J. Loeffel, rte de Sullens 8, 1030 Bussigny
B. Morel, rue Ecole-de-Médecine 16, 1205 Genève
P. E. Pilet, avenue Reymondin 13, 1009 Pully-Lausanne
W. Soerensen, (Les Chesaulx), 2036 Cormondrèche/Neuchâtel

Contents Sommaire Inhalt **Vol. 31 Fasc. 1-2 1977**

Avant-propos
Editorial du 1er numéro
S. Gagnebin, Le dialogue engagé
J. Piaget, Souvenirs sur Ferdinand Gonseth
Paul-Emile Pilet, Ferdinand Gonseth — Sa vie, son œuvre
S. Dockx, Note historique concernant la fondation de l'Union internationale de Philosophie des Sciences
Joel et Doris Jakubec, Gonseth, un protestant
Bernard Morel, L'ouverture à l'expérience spirituelle
Raymond Tschumi, Les esprits fins à la recherche de l'honnête homme
Eric Emery, La pensée de Ferdinand Gonseth et le temps dans l'art musical
Edmond Bertholet, La question de l'Etre chez Heidegger et chez Gonseth
F. Bonsack, La dialectique de la source et du filtre
Henri Lauener, Ferdinand Gonseth 1890–1975
Paul Bernays, Ueberlegungen zu Ferdinand Gonseths Philosophie
Jean-Blaise Grize, Discours, connaissance et actions
W. Soerensen, Ferdinand Gonseth et l'enseignement des mathématiques
Jean-Louis Destouches, Sur la méthodologie scientifique et les conceptions de Ferdinand Gonseth
Daniel Christoff, Raison suffisante et idonéisme, fermeture et ouverture
K. R. Popper, Some Remarks on Panpsychism and Epiphenomenalism
H. Guggenheimer, The Axioms of Betweeness in Euclid
Eva H. Guggenheimer, Magic Textiles

Notes de lecture

Paul-Emile Pilet, Pour une ouverture biologique à la psychologie de l'intelligence

Reviewing Studies — Etudes critiques — Betrachtungen zur Literatur

Jean Piaget, *Le comportement, moteur de l'évolution* (P. E. Pilet)
E. Jantsch and C. H. Waddington, *Evolution and Consciousness* (P. E. Pilet)
M. Schirn, *Studien zu Frege* (P. E. Pilet)
Science et Métaphysique, Bibliothèque des archives de philosophie (P. E. Pilet)
O. A. Johnson, *The Problem of Knowledge* (P. E. Pilet)
Patterns of Aging, Ed. H. Thomas Publ. S. Karger, Bâle (P. E. Pilet)

Subscriptions Abonnements Abonnemente	Switzerland Suisse Schweiz	Foreign Etranger Ausland
Subscription rate per annum (4 issues) Abonnement annuel (4 fascicules) Jahresabonnement (4 Hefte)	55.—Fr.s.	70.—Fr.s.
Price of single issues Prix du numéro Einzelnummer	14.—Fr.s.	18.—Fr.s.

Rédaction, Administration, Vente Redaktion, Verwaltung, Verkauf
Dialectica, Case postale 1081, 2501 Bienne (Suisse)
Dialectica, Postfach 1081, 2501 Biel (Schweiz)

Distribution/Auslieferung
Dialectica, Case postale 1081, 2501 Bienne (Suisse)
Stechert-Hafner Inc., 31 East 10th Street, New York 3, N.Y.
B. H. Blackwell Ltd., Broad Street, Oxford, England

RATIO

edited by STEPHAN KÖRNER

Volume XIX June 1977 No. 1

Emotions and Feelings	WILLIAM LYONS
A Place for Works of Art	T. J. DIFFEY
The Diversity of Identity	E. J. BOROWSKI
A Set of Criteria for Distinguishing Natural from Conventional Signs	EIKE V. SAVIGNY
Detecting Change	PAUL HELM
Our Considered Judgements	KAI NIELSEN
The Nature of Hume's Inductive Scepticism: A Critical Notice	JOHN CASSIDY
How to Derive 'Ought' from 'May'	WILLIAM G. LYCAN
On the Logic of Norms: A Natural Deontics	HELMUT ANGSTL
Towards a Satisfactory Formulation of Utilitarianism	R. I. SIKORA
Fairness and Fortune	J. P. DAY
On Wandschneider's Way Out	R. R. ROCKINGHAM-GILL

Annual Subscription: £9.25 (inland), £11.00 (overseas), $24.00 (U.S.A. and Canada)

Published by BASIL BLACKWELL, Oxford
Orders to: Journals Department, Basil Blackwell & Mott Ltd.,
108 Cowley Road, Oxford OX4 1JF, England.

Noûs

Nihil philosophicum a nobis putamus
Hector-Neri Castañeda, *editor*

Volume XI Number 2 May 1977

Henry E. Kyburg, Jr./ *A Defense of Conventionalism*
Tyler Burge/ *A Theory of Aggregates*
Robert Weingard/ *Space-Time and the Direction of Time*
Michael Jubien/ *Ontology and Mathematical Truth*
Jerome Gellman/ *The Meta-philosophy of Religious Language*
Richard E. Grandy/ *Predication and Singular Terms*
Ronald Scales/ *A Russellian Approach to Truth*

CRITICAL STUDY

Robert M. Adams/ The Nature of Necessity (*A. Plantinga*)
Announcements
Recent Publications in Philosophy

A quarterly, NOÛS is published in March, May, September and November. The March issue contains the major papers to be read at the Spring meetings of the Western Division of the American Philosophical Association. For *individuals* the subscription rates are: one year, $11.00; two years, $21.00; three years, $30.00. For *bona fide* students: $10.00. For institutions: $20.00 per year (plus 10% for subscriptions handled through a subscription agency). Please add $ .75 per year for all non-U.S. subscriptions.

Order from the Secretary, NOÛS
Department of Philosophy, 126 Sycamore, Indiana University
Bloomington, Indiana 47401

THE BRITISH SOCIETY FOR THE PHILOSOPHY OF SCIENCE

Committee 1977-8

President: Mr L. J. Cohen
Past President: Professor D. J. O'Connor
Hon. Secretary: Helena Sheiham
Hon. Treasurer: Mr D. W. Miller
Members: Dr D. Hirschmann, Mr C. Howson, Dr P. M. Williams, Mr A. Young

EDITORIAL BOARD OF THE JOURNAL

R. B. Braithwaite
H. Dingle
Sir John Eccles
M. B. Hesse
Sir Harold Jeffreys
W. C. Kneale
S. Körner
J. L. Mackie
Sir Karl Popper
H. R. Post
J. W. N. Watkins
G. J. Whitrow
J. H. Woodger

The British Journal for the Philosophy of Science is the official organ of the Society, whose purpose is described by the following statement from its Constitution and Rules:

'The purpose of the Society is to study the logic, the method and the philosophy of science as well as those of the various special sciences, including the social sciences.'

The Society holds meetings, at which papers are read and discussed, once a month during the academic year. These are held, by kind permission, in University College, London. The Committee may recognise Regional Groups of Members formed to hold meetings in other parts of the country. Any Regional Group so recognised may nominate a representative Member to the Committee.

Ordinary membership is open to all persons who have been proposed by two members, approved by the Committee, and elected at an ordinary meeting. Membership of the Society is international and is by no means confined to people resident in the United Kingdom. The annual subscription is £1·00 ($2.50). Those who wish to join should write to the Hon. Secretary, Helena Sheiham, Department of Philosophy, London School of Economics, Houghton Street, London WC2A 2AE.

Brit. J. Phil. Sci. Vol. 29 No. 1 March 1978

The British Journal for the Philosophy of Science

Contents

Articles

1–23 Spencer Graves On the Neyman–Pearson Theory of Testing

25–38 Richard Buxton The Interpretation and Justification of the Subjective Bayesian Approach to Statistical Inference

39–45 G. J. Whitrow On the Impossibility of an Infinite Past

Discussions

47–48 Karl Popper On the Possibility of an Infinite Past: A Reply to Whitrow

49–60 Elie Zahar Einstein's Debt to Lorentz: A Reply to Feyerabend and Miller

Review Articles

61–68 Roland Puccetti Unravelling the World Knot: Scientists and Philosophers on the Mind–Brain Controversy (review of G. G. Globus, G. Maxwell and I. Savodnik (*eds.*): *Consciousness and the Brain: A Scientific and Philosophical Inquiry*)

68–87 Donald Gillies The Development of Mathematics (review of M. Kline: *Mathematical Thought from Ancient to Modern Times*)

87–104 Raimo Tuomela Scientific Realism and Perception (review of J. W. Cornman: *Perception, Common Sense, and Science*)

Reviews

105–115 Alasdair MacIntyre on E. Gellner: *The Legitimation of Belief*; Edward Manier on F. J. Ayala and T. Dobzhansky (*eds.*): *Studies in the Philosophy of Biology, Reduction and Related Problems*

Announcements

Printed in Great Britain at the University Press, Aberdeen

Volume 29 Number 2 June 1978
BJPIA5 29 (No. 2) 117-212 (1978) ISSN 0007-0882

The British Journal for the Philosophy of Science

CALCUTTA
CENTRAL LIBRARY

Published for the
British Society for the Philosophy of Science
by Aberdeen University Press

THE BRITISH JOURNAL FOR THE PHILOSOPHY OF SCIENCE

Editors: JOHN WATKINS and JOHN WORRALL
Assistant Editors: COLIN HOWSON and ELIE ZAHAR

Subscriptions from January 1978
The *Journal* is published quarterly. The subscription price for mailing addresses in Britain is £7.00 net (U.S.A. and Canada U.S. $22.00, other overseas £11.00) per volume post free; single issues £2.50 net (U.S.A. and Canada U.S. $8.00, other overseas £4.00) plus postage. For members of the British Society for the Philosophy of Science—who should order direct from the publisher—the subscription price is £4.50 (U.S.A. and Canada U.S. $14.00, other overseas £7.00). Membership of the Society is international and is by no means confined to people resident in the United Kingdom. Orders may be sent to any bookseller or direct to Aberdeen University Press, Farmers Hall, Aberdeen, Scotland AB9 2XT.

Notes to Contributors
Articles, correspondence and books for review should be sent to John Worrall at the Department of Philosophy, London School of Economics, Houghton Street, Aldwych, London WC2A 2AE. Articles should be accompanied by abstracts of about 100 words.

Material is accepted for publication in the *Journal* on the condition that copyright is assigned to the British Society for the Philosophy of Science.

Three copies of all contributions should be sent, typed on one side only with wide margins and double-spaced. Footnotes, which should be as few as possible, should be typed separately, double-spaced at the end of the text. They should be referred to by consecutive numbering throughout the text, in which they will be inserted in page proof. Bibliographical references should be listed alphabetically at the end. The style of references should follow current *Journal* practice.

Long quotations are printed in small type without quotes, and should be so marked in the typescript. Single quotes should be used to form names of words and for shorter quotations; double quotes for other purposes. Diagrams, tables, and illustrations should be on separate sheets, with their desired position in the text clearly indicated. Symbolic formulae appearing as separate lines should be clearly distinguished in the text, and foreign letters (Greek, German, etc.) indicated in the margin. Material to be printed in italics, especially English letters used as symbols ('*A*', '*x*', etc.), should be underlined. On matters of style and layout, authors should consult CHAUNDY, T. W. *et al.* [1964]: *The Printing of Mathematics*. London: Oxford University Press.

Offprints of articles may be ordered when corrected galley proofs are returned. Fifty are supplied free ; the price of extra copies is given on the order form.

© British Society for the Philosophy of Science 1978

For permission to reproduce material from *The British Journal for the Philosophy of Science*, please apply to the Editors.

Back Volumes
Volumes 1-16 may be obtained from Kraus Reprint Ltd., FL-9491 Nendeln, Liechtenstein or Kraus Reprint Corporation, 16 East 46 Street, New York, N.Y. 10017. Volumes 17-27 may be obtained from Aberdeen University Press, Farmers Hall, Aberdeen, Scotland AB9 2XT. Prices on application.

Brit. J. Phil. Sci. 29 (1978), 117–130 *Printed in Great Britain*

Is Any of Popper's Arguments Against Historicism Valid?

by PETER URBACH

Introduction
1 *Argument One*
2 *Argument Two*
3 *Argument Three: Prophecy and Prediction*
4 *The 'Refutation of Historicism'*
5 *Can a Predictor Predict its Own Future Discoveries?*
Conclusion

INTRODUCTION

In his book, *The Poverty of Historicism*, Sir Karl Popper presents a famous polemic against the so-called doctrine of historicism. It is widely believed that Popper wreaked irrevocable damage on this doctrine.[1] In this paper I contend that Popper's arguments do not hit historicism at all. *My purpose is not to champion any particular historicist doctrine but to counter one of the most influential of many efforts which philosophers have made to refute the principle of the Unity of Method and to lay down special restrictions on research in the social sciences.*

Popper characterises historicism as:

> An approach to the social sciences which assumes that *historical prediction* is their principal aim, and which assumes that this aim is attainable by discovering the 'rhythms' or the 'patterns', the 'laws' or the 'trends' that underlie the evolution of history.[2]

A further crucial aspect of historicism is that in making statements about the future it employs 'unconditional historical prophecies' rather than 'scientific predictions' of the sort found in respectable physical sciences.

Popper provides three arguments against historicism in the main part of his book. I shall discuss these first and then consider his 'refutation of historicism' which appeared in the Preface to the 1957 edition of *The Poverty of Historicism.*

Received 5 *May* 1977

---

[1] For evidence of the great extent of this belief amongst philosophers and social scientists, see Suchting [1971], pp. 255–6.

[2] Popper [1957], p. 3.

I ARGUMENT ONE

Argument 1 against historicism is as follows:

> The search for the law of the 'unvarying order' in evolution cannot possibly fall within the scope of scientific method, whether in biology or in sociology. My reasons are very simple. *The evolution of life on earth, or of human society, is a unique historical process.* Such a process, we may assume, proceeds in accordance with all kinds of causal laws . . . Its description, however, is not a law, but only a singular historical statement. Universal laws make assertions concerning some unvarying order . . . and although there is no reason why the observation of one single instance should not incite us to formulate a universal law, nor why, if we are lucky, we should not hit upon the truth, it is clear that any law, formulated in this or in any other way, must be *tested* by new instances before it can be taken seriously by science. But we cannot hope to test a universal hypothesis nor to find a natural law acceptable to science if we are forever confined to the observation of one unique process.[1]

Popper here seems to assume that the historicist law is of the form $(x)Px$, where x ranges over complete evolutionary sequences. But, Popper avers, the universe of discourse contains only one element—the actual historical sequence—so that the historicist law must be untestable and therefore 'unacceptable to science'.

Popper then turns to two possible and rather obvious objections to his own argument.

(*a*) It might be denied that the evolutionary process is unique and instead the historicist might claim that the *same* evolutionary process can be observed in many different societies.

(*b*) The historicist could claim that 'in an evolutionary process, even if it is unique, we may discern a trend or tendency or direction, and that we may formulate a hypothesis which states this trend, and test this hypothesis by future experience'.[2]

Were either of these two objections sustained, then Popper's claim that historicist doctrines are not scientific would collapse. Popper tries to rebut these two objections. I shall argue that he does not succeed.

In discussing objection (*a*), Popper admits 'that history may sometimes repeat itself in certain respects', and that 'the parallel between certain types of historical events, such as the rise of tyrannies in ancient Greece and in modern times, can be significant for the student of the sociology of political power'. However, Popper objects, 'all these instances of repetition involve circumstances which are vastly dissimilar, and which may exert an important influence upon further developments'. Popper concludes that 'we have therefore no *valid* reason to expect of any apparent repetition of an historical development that it will *continue* to run parallel to its prototype'.[1]

---

[1] Popper [1957], pp. 108-9; my italics.

[2] *Ibid.*, p. 109.

Now Popper originally argued that no historicist theory is testable and the first objection to his argument claimed that historicist theories of the sort (x)Px; *may be testable when the variable ranges over observable historical sequences in different societies.* What Popper appears to be saying against this objection is that we cannot *validly infer* the historicist theory from observations of particular historical developments. It is, however, clear that this remark does nothing to establish that historicist theories must be *untestable* and it is odd, to say the least, to find Popper objecting to a theory on the grounds that it is not proved from facts. After all, the egregious feature of his philosophy is that the credit is given to just those theories which transcend the given facts and are as testable as possible. Nevertheless Popper repeats this ineffectual criticism in an attempt to meet objection (*b*). This second objection to Argument 1 envisages a situation in which a trend has been observed in some society—for example a trend over *n* years of increasing technological skill—and where the historicist simply generalises and proposes that the trend will continue indefinitely. (Admittedly, historicist theories of this type may be extremely unambitious and very likely false but the question at issue here is their falsifiability not their falsity or profundity.) Popper's rebuttal is based on the crucial difference between trends and laws. He emphasises that a description of the former is not a universal statement while a law is universal and that:

> The practical significance of this logical situation is considerable: while we may base scientific predictions on laws, we cannot (as every cautious statistician knows) base them merely on the existence of trends. A trend (we may again take population growth as an example) which has persisted for hundreds or even thousands of years may change within a decade, or even more rapidly than that.[2]

As a descriptive claim this is false for as Suchting nicely remarks: 'statisticians such as actuaries, sufficiently cautious to make large profits for insurance companies, do predict on the basis of trends'.[3]

---

[1] Popper [1957], p. 111; my italics replace those in the original. It may be that Popper intends to indict historicism on slightly different grounds for he remarks at this point that the historicist theory may *appear* to be highly confirmed by practitioners carefully selecting or reinterpreting, *ad hoc*, refuting evidence. He particularly indicts Toynbee's theory on this count. But clearly these remarks can only be criticisms of *historicists* rather than specific objections to historicist *theories*. They point to a danger in investigations in all areas, including physics. Moreover, demonstrating that Toynbee's theory is refuted demonstrates that it is refutable and so destroys the claim in Argument 1. Similar *ad hominem* arguments are offered by Popper in his [1957], section 28. See p. 123, n. 5, *below*.

[2] Popper [1957], p. 115.

[3] Suchting [1971], p. 259. Criticisms of Popper's Argument 1 along the same lines are presented in Suchting [1971], pp. 236–40.

If on the other hand, Popper is prescribing right scientific behaviour then the only foundation which I can discern for the precept is that a 'trend which has persisted for hundreds or even thousands of years may change within a decade or even more rapidly than that' and hence that the generalisation stimulated by observing the trend may be false. Clearly this argument merely repeats the triviality that universal historical theories cannot be logically inferred from statements describing trends. Hence it does not even impinge on the claim that historicist theories are ***unscientific*** that is, untestable, the claim which Argument 1 was intended to establish.

2 ARGUMENT TWO

Let us now consider Argument 2 against historicism. This argument seeks to establish, not the unscientific character of historicist laws, but their falsity.

> *The crucial point is this:* although we may assume that any actual succession of phenomena proceeds according to the laws of nature, it is important to realise that practically no sequence of, say, three or more causally connected concrete events proceeds according to any single law of nature. If the wind shakes the tree and Newton's apple falls to the ground, nobody will deny that these events can be described in terms of causal laws. But there is no single law, such as that of gravity, nor even a single definite set of laws, to describe the actual o concrete succession of causally connected events; apart from gravity, we shou have to consider the laws explaining wind pressure; the jerking movements the branch; the tension in the apple's stalk; the bruise suffered by the appl on impact; all of which is succeeded by chemical processes resulting from the bruise, etc. The idea that any concrete sequence or succession of events (apart from such examples as the movement of a pendulum or a solar system) can be described or explained by any one law, or by any one definite set of laws, is simply mistaken. There are neither laws of succession nor laws of evolution.[1]

It is difficult to understand Popper's argument here as it appears at first sight to be inconsistent. We are asked to consider a sequence of events involved in the fall of Newton's apple. Now this phenomenon can be explained in terms of a set of laws $\{L_i\}$ and a set of initial conditions $\{I_i\}$. But, Popper states, there is no 'single law, such as that of gravity, nor even a single definite set of laws to describe the actual or concrete succession of causally connected events'. This statement sounds odd in conjunction with the claim that the set of laws $\{L_i\}$ *can* explain the succession of events of Newton's falling apple, and suggests that Popper wants to say that no set of laws *alone*, that is without the aid of initial conditions, can explain the events comprising the apple's fall. But if Popper thinks that historicists purport to explain sequences of events using universal laws *alone*, then his criticism could have been very much more direct—the historicists would be guilty of logical errors, since they could not possibly derive observational consequences from their theories without employing some initial or boundary conditions.

---

[1] Popper [1957], p. 117; my italics replace those in the original.

The historicist theories (or 'laws of succession' or 'laws of evolution') mentioned by Popper are substitution instances of the following:

> All human social formations of type T pass through stages described by the statements S_1 S_2 . . ., S_n in a given temporal order.

Let us take the typical historicist claim to be of this form. In his Argument 2, Popper is asserting that, with a few minor exceptions to be discussed below and encountered only in physics, no concrete sequence of events can be described or explained by any set of laws. Now this claim would be clearly false if, by 'law', we understood a conjectural universal theory because we can always construct a universal statement one of whose consequences, in the presence of suitable initial conditions, describes a given finite sequence of events. And we just do not know whether this theory will be independently confirmed, nor will any amount of *a priori* cogitation tell us. But perhaps Popper intends to deny that any single law, *qua true universal statement* can explain any given, concrete sequence of historical events. But if so it is difficult to see what would justify this denial. Popper seems to assume that if a sequence of events is determined by initial conditions and a set of (true) laws then there can be no single law (true universal statement) which describes that sequence. But this is not correct. Suppose, for example, that the metabolic processes of all animals are determined by physical and chemical laws together with appropriate initial conditions describing, for example, the local vegetation. Then there may be a countably infinite number of true, strictly universal laws describing sequences of events in the animal kingdom. 'All Moas die before reaching the age of fifty' is probably one of these.

My last example recalls Popper's discussion of physical necessity where this statement about Moas is proposed as a 'true, strictly universal statement which ha[s] an accidental character rather than the character of true universal laws of nature'.[1] This example suggests to Popper that 'the characterisation of laws of nature as strictly universal statements is logically insufficient and intuitively inadequate'.[2]

Popper's claim that laws of nature are in some sense 'necessary' suggest a third interpretation of his anti-historicist argument in which it may be taken as challenging historicist pretensions to be able to discover physical necessities in history or 'inexorable laws of succession'. Thus perhaps

---

[1] Popper [1959], p. 427.

[2] Popper [1959], p. 428.

Popper will admit that an historicist may alight on a true universal statement of social evolution in *this* world but that this statement would fail in some or all those possible worlds 'which differ from our world, if at all, only with respect to initial conditions'.[1]

If this is Popper's claim then it is hardly likely to terrify any historicist, the scientific character of whose theories will not be diminished. It seems likely that Kepler's Laws, for example, are mere accidental regularities which, but for the energy which the heavenly bodies each *happen* to have acquired at the beginning of time, would not hold. It is doubtful, however, whether any philosopher has ever condemned or dispraised Kepler's work on this account.

But more importantly, if we interpret Popper's argument as an attempt to show that no historicist theory can be a naturally necessary law, then we will not have rescued it. For suppose that a historicist truly proposes that every society *in this world* proceeds through a certain sequence of states $S_1, \ldots S_n$ in the given temporal order. Now even if every instance of $S_1, \ldots S_n$ is determined by a set of initial conditions $\{I_i\}$ which is realised at the beginning of the universe and a set of physically necessary laws $\{L_i\}$, it is logically possible for the same sequence of states to occur in the same temporal order in every other world which differs if at all with respect to the $\{I_i\}$.

It is thus entirely a matter for conjecture whether an historicist theory applies in other worlds and in this respect such theories are on a par with theories in physics.

In his defence of Argument 2 which I quoted at length above, Popper allows a couple of exceptions to the principle that no concrete sequence of events can be explained by any law of the historicist type. In particular he allows that the succession of events comprising the motion of a pendulum and the solar system may be so explained. No argument is here supplied to underwrite any crucial difference between such systems and human societies. In the next section I consider an argument Popper does provide for the existence of an important difference between solar systems and human societies. We will see that this however begs the question.

3 ARGUMENT THREE: PROPHECY AND PREDICTION

That Popper himself regards neither Argument 1 or 2 as conclusive emerges from his admission some pages after defending them that his 'criticism of the doctrine of historical laws of succession is in one important respect still inconclusive'.[2]

---

[1] Popper [1959], p. 433.

[2] Popper [1957], p. 120

Popper now considers a possible counter-argument to his claim that a 'trend, as opposed to a law, must not in general be used as a basis for scientific predictions'.[1]

The counter-argument Popper considers is as follows:

> should we succeed in reducing a trend to a set of laws [that is, explaining the trend using laws], then we should be justified in using this trend, like a law, as a basis of predictions.[2]

Popper admits that explained trends do exist but that 'their persistence depends on the persistence of certain specific initial conditions'.[3] The great fault of historicists is that they

> *overlook the dependence of trends on initial conditions.* They operate with trends as if they were unconditional, like laws. Their confusion of laws with trends makes them believe in trends which are unconditional (and therefore general); or, as we may say, in '*absolute trends*' . . .
>
> *This, we may say, is the central mistake of historicism.* Its 'laws of development' turn out to be absolute trends; trends which, like laws, do not depend on initial conditions, and which carry us irresistibly in a certain direction into the future. They are the basis of unconditional prophecies, as opposed to conditional scientific [*sic*] predictions.[4]

What exactly does this criticism amount to? Popper seems to be claiming that

(*i*) there are two types of prediction—unconditional prophecy and conditional prediction;

(*ii*) prophecies are unscientific while predictions are scientific;

(*iii*) historicism necessarily operates with prophecies;

(*iv*) hence, historicism is not scientific.[5]

---

[1] *Ibid.*

[2] *Ibid.*, p. 121.

[3] *Ibid.*, p. 128.

[4] Popper [1957], p. 128; my italics replace those in the original.

[5] See also Popper's [1945] (vol. 1, p. 3) where he says that the claims of historicism 'in spite of their plausibility . . . are based on a gross misunderstanding of the method of science, and especially on the neglect of the distinction between scientific prediction and historical prophecy'.

Popper's words in the section from which the above quotations are taken often suggest that he has in mind a modification of (*iii*), namely that *historicists* have a psychological proclivity to lapse into pseudoscience. For example, in summing up section 28 in his [1957] in which these arguments occur, Popper says:

> There are indeed, countless possible conditions [determining a trend]; and in order to search for the true conditions of a trend, we have all the time to try to imagine conditions under which the trend in question would disappear. But this is just what the historicist cannot do. He firmly believes in his favourite trend, and conditions under which it would disappear are to him unthinkable. The poverty of historicism, we may say, is the poverty of imagination. The historicist continually upbraids those who cannot imagine a change in their little worlds; yet it seems that the historicist is himself deficient in imagination, for he cannot imagine a change in the conditions of change.

No doubt there are such historicists at large and it is to be regretted; but this hardly constitutes grounds against historicism or any objective historicist theories. It clearly has no status as a criticism of historicism. Such *ad hominem* arguments are not isolated. See, for example, *above*, p. 119, n. 1.

Popper's own account of prophecies and predictions seems to belie this interpretation of his third argument against historicism. In his earlier discussion of the distinction between prophecies and predictions Popper talks of 'two different kinds of predictions [which] can be distinguished *in science*'[1] and these he calls prophecies and '*technological* predictions'. Thus, according to this characterisation prophecies are sometimes *legitimately* employed within the respectable sciences. This implies the falsity of (*ii*).

Popper tells us that an instance of a prophecy is 'the coming of a typhoon' and a typical technological prediction is 'if a certain shelter is to stand up to a typhoon, it must be constructed in a certain way, for instance with ferroconcrete buttresses on its north side'.[2] The crucial distinction between these two types of prediction is that in the case of prophecies 'we are told about an event which *we can do nothing to prevent*' while technological predictions 'form a basis of *engineering* . . . intimating steps open to us *if* we want to achieve certain results'.[3] Both types of prediction are derived from laws and initial conditions and in one case, but not the other, whether the initial conditions actually obtain or not is independent of human actions. It is clear that, as so far characterised, there is nothing unscientific about 'prophecies', for all the word's Old Testament associations.[4] It is odd therefore that when criticising historicism a few chapters after the discussion of prophecies and technological predictions Popper talks of 'prophecies' and '*scientific* predictions', thereby suggesting without argument that the former type of prediction is unscientific.

Perhaps, the explanation for this terminological metamorphosis can be found in Popper's [1948] essay on 'Prediction and Prophecy in the Social Sciences'. Here he contends that there are special circumstances which sometimes licence the use of prophecies and that although these are encountered from time to time in the natural sciences, they are never found in the social sciences. In particular,

> long term prophecies can be derived from scientific conditional predictions only if they apply to systems which can be described as well-isolated, stationary and recurrent. These systems are very rare in nature, and modern society is surely not one of them.[5]

It is difficult to see what exactly this claim amounts to. The terms 'well-isolated', 'stationary' and 'recurrent' are significant only when it is specified from what influences the system is isolated and with respect to what properties it is stationary and recurrent. Certainly, the solar system is (according to current theories) approximately stationary with regard to mechanical energy and angular momentum and recurrent with respect to the position and momentum of its constituent planets, and it is well-isolated from certain specified possible influences on these positions and momenta. But, with respect to many chemical and biological-properties (for example, the number of human beings), it is not recurrent; nor is it well-isolated from some external influences (for example, star light) on these properties. Human societies are also well-isolated and recurrent in some respects, but not in others. Whether human societies are well-isolated with respect to influences which would make all historicist theories false is implausible: more importantly it is precisely the point at issue in the argument over historicism and cannot be decided by any amount of *a priori* cogitation. Nothing in Popper's arguments so far considered decides this central question either way.

---

[1] Popper [1957], p. 42; my italics.

[2] *Ibid.*, p. 43.

[3] *Ibid.*, p. 43; my italics.

[4] Nor is the distinction between prophecies and predictions time-independent. With increasing human knowledge and power prophecies may turn into predictions.

[5] Popper [1948], p. 339.

4 THE 'REFUTATION OF HISTORICISM'

In the Preface to the 1957 edition of *The Poverty of Historicism*, Popper makes the following bold claim:

> I tried to show, in the *Poverty of Historicism*, that historicism is a poor method—a method which does not bear any fruit. But I did not actually refute historicism. Since then, I have succeeded in giving a refutation of historicism: *I have shown that, for strictly logical reasons, it is impossible for us to predict the future course of history.*[1]

The 'refutation' proceeds as follows:

(*i*) The course of human history is strongly influenced by the growth of human knowledge.

(*ii*) We cannot predict, by rational or scientific methods, the future growth of our scientific knowledge.

(*iii*) We cannot, therefore, predict the future course of human history.[2]

Now this argument sounds very plausible as it stands but it clearly depends for its validity on the following tacit assumption:

(*iia*) *We cannot predict, by rational or scientific methods, events which are strongly influenced by events which cannot be so predicted.*

The first premiss in Popper's argument is uncontroversial. Popper claims that the second—'the decisive step in the argument'—is also beyond dispute since it has been '*logically proved*'. However I do not think that this premiss can be accepted. As I shall show in the next section, *the 'logical*

---

[1] Popper [1957], p. *v*.

[2] *Ibid.*, pp. *v–vi*.

proof' for (ii) is either invalid or else irrelevant to the argument against historicism.

Let us first examine premiss (*iia*). Popper leaves this as an unstated and unsupported assumption. It is however not true since, *clearly, the fact that each member of a sequence of events is strongly influenced by unpredictable occurrences is compatible with the existence of regularities—arising by accident, as it were—in the sequence.* Indeed, we seem to accept some laws in physics but nevertheless acknowledge that these laws reflect only the broad characteristics of sequences of events which themselves are unpredictable. Boyle's law is a simple example of this. It relates rather general features (pressure and volume) of a sequence of states of interacting particles, none of which is individually predictable. Moreover, it is often admitted that this unpredictability of the micro-events may be insurmountable in principle, without the truth of the macro-laws thereby being put in question.

Similarly, if the final chapter of Man's existence is ever written the divine mind will certainly be able to discern universal regularities of a historicist kind on perusing the now completed book of human history. Whether such regularities actually exist and can be formulated as historicist laws is quite independent of whether the recurring events were determined by *individually unpredictable* events. The suppressed assumption (*iia*) in Popper's supposed refutation of historicism is therefore not true.

One might, nevertheless, be inclined to think that the tacit assumption in Popper's 'refutation', while not being a logical truth, is at least a highly plausible assumption about the world. That is, one might credit the appearance of a reasonably simple historicist law about events which are strongly influenced by unpredictable events with such small probability that he might feel that the possibility can safely be discounted altogether. Popper's argument could then be taken as no logical proof but nevertheless as a very good argument which establishes its antihistoricist conclusion with very high probability. However, I do not think that this weakening of the claim against historicism will do. First, the probability of *any* law statement is rather small and is (as Popper himself says) probably best regarded as being zero. Historicist theories would therefore be no more improbable than any law statement in the physical sciences. Secondly, as I have already mentioned, there are reliable and well attested macrolaws in physics which, like historicist laws, relate broad features of a system while the more fundamental parts of that system are (perhaps in principle) unpredictable. And for all we can tell, *every* law of physics is of this sort.

5 CAN A PREDICTOR PREDICT ITS OWN FUTURE DISCOVERIES?

As we have seen, Popper's final and best regarded attempt to *refute* historicism failed because an important but tacit assumption in that argument is false.

It is, I think, of interest to pay some attention here to the second premiss in Popper's argument—a premiss which Popper claims to be 'logically proved' and which is often thought to set important limits on our powers of scientific prediction, in particular within the social sciences.[1] It thus seems to me worth discussing.

The premiss in question states that 'we cannot predict, by rational or scientific methods, the future growth of our scientific knowledge'. Two arguments are offered for this, one formal and the second informal. Since only the first is regarded by Popper as amounting to a logical proof,[2] I will deal with it first.

Popper's 'proof' consists in showing that '*no scientific predictor*—whether a human scientist or a calculating machine—*can possibly predict, by scientific means, its own future results.* Attempts to do so can attain their result only after the event, when it is too late for a prediction; they can attain their result only after the prediction has turned into a retrodiction'.[3] For the putative proof for this claim we are referred to Popper's well-known [1950] article on 'Indeterminism in Classical Physics and in Quantum Physics' and the nub of the argument is as follows: It is a consequence of Gödel's second incompleteness theorem that in any consistent system sufficiently rich to incorporate arithmetic there are well-formed formulas, f, such that f is not a theorem and the formula corresponding to 'f is not a theorem' is also not a theorem. An example of such a formula is the Gödel sentence G. Popper now considers a class of questions which might be put to a physical predictor, the following being a possible example of a question which is put at time t_1:

> If you are asked, at t_2, the question "is G a theorem?" (and allowed time until t_3 for an answer) will you or will you not before t_3 react with "yes" or "no"?[4]

Popper asserts that if the predictor instantiates a consistent system it cannot answer this question before t_3. Only after t_3, after it has lost its predictive role, will the predictor answer the question—with 'no'.

This argument purports to show that there is at least one future physical state of the predictor which it is unable to predict by rational means. Now *ex hypothesi*, since it is consistent, the predictor will not be able to prove the correct reply to the question before t_3. So the future state which the predictor is allegedly unable to predict is the *absence* of any response to a certain question. This non-response can hardly be counted as a *discovery* and it is difficult to see exactly why this argument of Popper's is thought relevant to the claim that predictors cannot predict their own future *discoveries*.

---

[1] See for example, Winch [1958], pp. 93–4.

[2] Popper [1957], p. *vi*.

[3] *Ibid.*, pp. *vi–vii*.

[4] *Cf.* Popper [1950], p. 183.

One might have thought that the weakness in the argument could be remedied by considering an actual discovery of the predictor, that is to say, a theorem of the logical theory which our predictor instantiates. But such an adjustment does not save Popper's argument. If g is a theorem of the system, then if the system is sufficiently rich to incorporate recursive arithmetic the theorem corresponding to 'g is a theorem' is also a theorem. Thus, the undecidability of our system does not preclude the prediction of the discovery of a theorem.

There is a further weakness in Popper's argument. Suppose that there were theorems (discoveries) which a predictor makes but no proof that the predictor will make them. Can it however now be inferred that we cannot predict, *by rational or scientific methods*, the future growth of our scientific knowledge? Well, it depends on the gloss we give to the italicised phrase. If a prediction is scientific only if it was proved from axioms, then Popper's conclusion does follow.

But it is at the core of Popper's philosophy that 'rational and scientific predictions' are *not proofs but are fallible and based on conjectures*. If we accept Popper's characterisation of science. Then clearly Popper's argument could not be valid.

To return to our previous example, there is no reason why the predictor should not *conjecture* that it will prove (or fail to prove) g, or G, to revert to Popper's example. This conjecture might even be independently corroborated and may actually be true.

I conclude that the undecidability of first order systems does *not* imply that a predictor cannot scientifically predict its own future discoveries.

The second, informal, argument Popper suggests against the possibility of predicting the future growth of scientific knowledge is this:

> *If there is such a thing as growing human knowledge, then we cannot anticipate today what we shall know only tomorrow.*[1]

Popper says that he regards this as 'convincing' and 'sound reasoning', although not a logical proof. It does not seem to me to be even sound reasoning. Put a bit more precisely, Popper's claim comes to this: if a predictor predicts at time t_1, that it will discover a scientific theory at time

---

[1] Popper [1957], p. *vi*.

t_2, then the prediction will be self-refuting since the discovery of this theory will have been made already at t_1.

Now this last argument assumes that it is a *logical truth* that if *P predicts* at t_1 the discovery of theory X, then *P discovers* X at t_1. However, if we stick to everyday usages of the terms 'predict' and 'discover' this assumption is unwarranted. Consider the following possibility. Predicting a human discovery by scientific means, should it ever be possible, is likely to involve considerable computation for which an electronic computer would be needed. A human scientist might set about predicting his own future discoveries by feeding relevant initial conditions into the computer together with the appropriate theories. Suppose now that the scientist does not decode the information on the output tape. Then, I take it, the scientist has not *discovered* the information contained on the output tape for, at least in ordinary usage, one discovers information only if he becomes aware of that information. Nevertheless, the scientist will have *predicted* the information on the output tape; and moreover, the prediction will not be self-refuting. Thus, *pace* Popper, there is no logical impossibility of a predictor scientifically predicting its own discoveries. One could perhaps rescue Popper's argument by stipulating that 'if P predicts at time t_1 the discovery of theory X, then P is aware at t_1 of the theory X' is true by definition of the word 'predict'. But this peculiar modification of English usage would make Popper's argument quite irrelevant to the question of historicism, since, in making an historicist prediction one does not have to *become aware* of any theories whose discovery influences the predicted social event. For suppose that—unlike with any known historicist theory—the predictions of future social occurrences involve the prior prediction of some influential scientific developments. The predicted event might then be the value of the function $f(x_1, \ldots, x_n, f'(y_1 \ldots y_m))$, where the value of $f'(y_1 \ldots, y_m)$ describes the relevant scientific development. It is clear that by employing a suitably programmed computer the historicist prediction could have been carried through without any conscious mind contemplating the results of any of the intermediate steps. Thus the prediction of a future social event could have been successfully carried out even though that event was crucially affected by some human scientific discovery.

Popper's arguments that no predictor can predict its own future discoveries are thus either invalid or irrelevant to the issue of historicism.

CONCLUSION

I have reviewed Popper's attempts to demolish historicist theories and to lay down special restrictions on the predictive power of the social sciences. I conclude that none of Popper's arguments is valid, and that therefore the principle of the unity of the natural and the social sciences is unimpugned by these arguments.

London School of Economics

REFERENCES

Popper, K. R. [1945]: *The Open Society and its Enemies.*

Popper, K. R. [1950]: 'Indeterminism in Quantum Physics and in Classical Physics', *British Journal for the Philosophy of Science,* **1**, pp. 117–33 and 173–95.

Popper, K. R. [1957]: *The Poverty of Historicism.*

Popper, K. R. [1959]: *The Logic of Scientific Discovery.*

Popper, K. R. [1963]: *Conjectures and Refutations.*

Popper, K. R. [1948]: 'Prediction and Prophecy in the Social Sciences'. Reprinted in Popper [1963], pp. 336–46.

Suchting, W. A. [1971]: 'Marx, Popper and "Historicism"', *Inquiry* **15**, pp. 235–36.

Winch, P. [1958]: *The Idea of a Social Science and Its Relation to Philosophy.*

Brit. J. Phil. Sci. 29 (1978), 131–143 *Printed in Great Britain*

A Defence of Evolutionary Laws

by A. OLDING

That there are no laws of historical development or of biological evolution has, it is widely thought, been decisively shown by Sir Karl Popper. In what follows I shall be concerned mainly with his argument as it appears in *The Poverty of Historicism*[1] and, in particular, as it is supposed to count against the possibility of laws of biological evolution. However, although my concern is mainly with biology, much of what I say applies also to history and the possibility of laws of historical development. This is obviously true if I succeed in showing that Popper's arguments are generally invalid (a task made easier by the fact that he explicitly states that they are directed against the possibility of both historical and biological evolutionary laws); but it is also the case in so far as, in my positive suggestions in support of the possibility of evolutionary laws in biology, explanations of biological evolution can be said to 'map' those of historical development. With regard to this last point, I shall not, in this paper, make explicit what important similarities might exist between biological and historical explanations but simply record my belief that there are such similarities and note that Popper also believes this.

Popper answers the question 'Can there be a law of evolution?' as follows (pp. 108–9):

> I believe that the answer to this question must be 'No', and that the search for the law of the 'unvarying order' in evolution cannot possibly fall within the scope of scientific method, whether in biology or in sociology. My reasons are very simple. The evolution of life on earth or of human society, is a unique historical process. Such a process, we may assume, proceeds in accordance with all kinds of causal laws, for example, the laws of mechanics, of chemistry, of heredity and segregation, of natural selection, *etc.* Its description, however, is not a law, but only a singular historical statement. Universal laws make assertions concerning some unvarying order, as Huxley puts it, *i.e.* concerning all processes of a certain kind; and although there is no reason why the observation of one single instance should not incite us to formulate a universal law, nor why, if we are lucky, we should not even hit upon the truth, it is clear that any law formulated in this or in any other way, must be *tested* by new instances before it can be taken seriously by science. But we cannot hope to test a universal hypothesis nor to find a natural law acceptable to science if we are for ever confined to the observation of one unique process. Nor can the observation of one

Received 1 *June* 1977

---

[1] K. R. Popper [1957]. Page references in the text are to this work.

unique process help us to foresee its future development. The most careful observation of *one* developing caterpillar will not help us to predict its transformation into a butterfly.[1]

Central to Popper's argument, it seems to me, is the alleged uniqueness of the evolutionary process. The underlying argument, I think, is this: 'The evolution of life on earth . . . is a unique historical process' therefore 'Its description is not a law, but only a singular historical statement.' This argument, if valid, establishes that there cannot be a law of evolution because such a law would have to be universal. As I say, this seems to me to be the fundamental argument but added to it is the following: '. . . any law . . . must be *tested* by new instances before it can be taken seriously by science'. But as 'we are for ever confined to the observation of one unique process' a proposed universal statement concerning it cannot be tested. Therefore, if we put forward such a universal statement it cannot 'be taken seriously by science'. Added to this again is the argument that because we have only 'the observation of one unique process' we cannot foresee its future development.

As I have formulated Popper's argument its core is pretty clearly ontological: there are no laws of evolution because of the fact of the uniqueness of the evolutionary process. However, this is not the only interpretation possible. W. A. Suchting, in his own criticism of Popper,[2] has claimed that the latter's argument is essentially epistemological. The weight of the argument lies not in its disproof of general laws of development, but only in its proof that such laws could not be scientific. According to this interpretation, then, Popper believes that the proponent of evolutionary laws mistakes a singular statement of an evolutionary sequence for a law which requires a universal statement for its expression. Further, if the proponent of evolutionary laws tried to restate such a singular statement as a universal statement then 'Popper's criticism of this move is that such a universal could not be a scientific law, if only because there is but one sequence instantiating it (*viz*. the actual historical sequence), whereas scientific laws require a multiplicity of testing possibilities.'

Suchting claims of this argument that it 'is epistemic or methodological: it may be taken to deny the existence of scientific (empirical, falsifiable) laws of historical development and not of laws of development in general.'[3]

---

[1] Despite the difficulty of its interpretation, this seems to me to be Popper's fundamental argument against evolutionary laws (it is certainly his most *general* argument). I therefore find it surprising that, in his [1973] book on Popper, Bryan Magee makes no mention of it and indeed refers to the more *particular* argument 'that no scientific predictor, whether a human scientist or a calculating machine, can possibly predict, by scientific methods, its own future results' (Magee, p. 100). Popper gives a *sketch* of this argument in the introduction to *The Poverty of Historicism* but his promise (1957) to publish a fully worked-out version is yet to be fulfilled.

[2] W. A. Suchting [1972], p. 237.

[3] *Ibid.*, p. 238.

Even if we were to accept Suchting's epistemological interpretation of Popper's argument, we should still question the argument's validity. It does not follow from the fact that there is only one instance of something that we cannot formulate general statements about it which allow for 'a multiplicity of testing possibilities'. Indeed, it is arguable that because statements of scientific laws are hypotheticals—they are of the form '*If* X *then* Y'—such laws can be both true and have empirical consequences even though the things they are about do not exist.[1] For example, the number of possible proteins is much greater than the number of proteins that has ever existed[2]; but there is nothing, apart from the slowness or dimness of our wits (which we might therefore replace with a computer), to stop us formulating possible general laws concerning such a non-existent protein. And such a law might have empirical consequences, for example, by entailing, in conjunction with other statements, that certain breakdown products of this protein would have such and such properties—the breakdown products might be synthesised and their properties ascertained. If we can quite properly formulate laws for non-existent things, then I take it we may also be able to formulate them for things that exist but once. Nor, as far as this interpretation of Popper's argument is concerned, is the logical situation different for single developmental sequences. The difference here between finding the laws governing the activities of a single protein molecule and those governing the transformation of Popper's lonely caterpillar into a butterfly is one of complexity only. In principle we might stumble upon the law that earthbound caterpillars become butterflies and the statement of this law might have consequences that do not require us to wait and see this happen; for example, that this particular hormone the caterpillar possesses has such and such an effect on isolated colonies of cells.

However, I think that Suchting's interpretation of Popper is mistaken although understandably so.[3] My own reasons for taking the ontological interpretation—that because the evolutionary sequence is unique there are no laws of development of any kind, empirical or otherwise—are as follows. First, this is how Popper, at the conclusion of the paragraph stating his argument, sums things up.

> As applied to the history of human society—and it is with this we are mainly concerned here—our argument has been formulated by H. A. L. Fisher in

---

[1] Popper's way of putting it has the same consequence. For example: 'A universal law, on the other hand, does not assert existence; on the contrary: as was shown at the end of section 20, it asserts the impossibility of something or other' (p. 115).

[2] For this claim see, for example, John Maynard Smith [1972], p. 92.

[3] Or it may be that we are both right in that Popper has mixed together two quite separate arguments.

these words: Men ... have discerned in history a plot, a rhythm, a predetermined pattern ... I can see only one emergency following upon another ..., only one great fact with respect to which, since it is unique, there can be no generalisations ... (p. 109).

Here the argument is clearly as I have suggested: because history (or evolution) is unique 'there can be no generalisations'. Second, in the paragraph that is the lead up to his main argument, Popper is anxious to show that the 'evolutionary hypothesis', that is, the apparently universal statement, despite appearances to the contrary is really a singular statement. Thus he argues that we may be misled by the word 'hypothesis' because we often use it 'to characterise the status of universal laws of nature' (p. 107). 'Hypothesis' is used 'in a different sense'[1] about evolution where, as in medical diagnosis, it is singular. Also, in a footnote to this section, he describes how, in a different way, a singular statement may take on the *appearance* of a universal law: 'Even a statement such as 'All vertebrates have one common pair of ancestors" is not, in spite of the word "all", a universal law of nature; for it refers to the vertebrates existing on earth, rather than to all organisms at any place and time which have that constitution which we consider as characteristic of vertebrates'. Third, the fundamental importance for Popper's argument of the alleged uniqueness of the evolutionary or historical process is shown by the fact that he considers an important objection to it would be the reply that such processes are not unique, that they may be cyclical, and he devotes some space to refuting this (p. 109).

I shall, then, leave the further refutation of the Suchting interpretation of Popper to Suchting[2] and concentrate on my own. According to this Popper's argument is that there can be no historical or evolutionary laws because of the uniqueness of the relevant sequences. This immediately raises the question of what is meant here by 'unique'. There are many ways in which an event or process can be described as 'unique' and, as R. Gruner points out,[3] such a description does not automatically entail that the event or process is not law-governed. For example, perfectly law-abiding events may have unique spatio-temporal coordinates.

The uniqueness, then, must be of a particular kind. It must be that in those respects which might allow for the pinning down of historical laws historical and evolutionary processes are unique—they only happen once. For example, if we look at evolution we might put forward as a candidate for a universal law 'Fish beget amphibians and amphibians beget reptiles and reptiles beget birds and mammals'; but if we could wipe the slate clean and start the evolutionary process afresh, quite different beasts would populate the earth or, at least, if the evolutionary sequence did repeat itself this would be somehow accidental.

---

[1] In his [1972] Popper says, 'One should never get involved in verbal questions or questions of meanings and never get interested in words' (p. 310).

[2] In fact, Suchting is mainly concerned to show that the views Popper attributes to Marx were not held by the latter.

[3] R. Gruner, [1969], pp. 145-46.

I take it that this uniqueness is not a matter of logic (although it may be a matter of logic that in some respects all things are unique and in other respects are not unique) for I can perfectly well imagine history or evolution repeating itself. It must, then, be a factual uniqueness. But why should we believe in this factual uniqueness? Popper has two arguments which bear on this question. It will be convenient to reverse the order of his arguments and consider first his attack on the belief that 'we may discern, and extrapolate, the trend or direction of an evolutionary movement' (p. 112). His argument here is complex but we can single out what he, himself, refers to as the 'crucial point' (p. 117):

> The crucial point is this: although we may assume that any actual succession of phenomena proceeds according to the laws of nature, it is important to realize that practically *no sequence of, say, three or more causally connected concrete events proceeds according to any single law of nature.* If the wind shakes a tree and Newton's apple falls to the ground, nobody will deny that these events can be described in terms of causal laws. But there is no single law, such as that of gravity, nor even a single definite set of laws, to describe the actual or concrete succession of causally connected events; apart from gravity, we should have to consider the laws explaining wind pressure; the jerking movements of the branch; the tension in the apple's stalk; the bruise suffered by the apple on impact; all of which is succeeded by chemical processes resulting from the bruise, *etc.* The idea that any concrete sequence or succession of events (apart from such examples as the movement of a pendulum or a solar system) can be described or explained by any one law, or by any one definite set of laws, is simply mistaken. There are neither laws of succession, nor laws of evolution.

The thought seems to be that, unlike pendulums and the solar system, the evolutionary sequence in having to obey such a jumble of laws is, naturally enough, lacking in repeatable order. We can, I think, bring out the point of Popper's argument by considering the most famous of Auguste Comte's laws of succession, the Law of the Three Stages, in which he envisages the intellectual history of the human race as in some sense necessarily proceeding first through the theological stage, then the metaphysical stage and finally arriving at the positive stage. Popper's argument is that even if there is such a Law of the Three Stages, it could not, *by itself*, determine the course of history. It would be just one among many laws in accordance with which history proceeds in the messy way that Popper and Fisher thinks it does. To think that the Law of the Three

Stages somehow describes the way history has actually gone—in that there actually were theological, metaphysical and positive stages—is to think that this law can, as it were, override other laws. And this is to make the same mistake as someone who believes he can account for a falling apple in terms of the law of gravity alone, as if the law of gravity overrides the laws of friction, pressure, *etc.*

If this is Popper's argument, then it is hard to see to whom it would apply. Perhaps there are some people who believe that history or evolution go on inexorably regardless of the rest of the universe—some religious people, perhaps, who, believing that God provided the laws of the universe, can if he wishes, also ride roughshod over them—but we should not, I think, include Comte in such company and certainly not most biologists who believe in evolutionary laws. There is no battle of the natural laws, no survival of the fittest where the toughest law lords it over the rest. There are just the bits of the universe going on in accordance with whatever laws happen to be around. But this does not entail that a sequence of events, such as evolution, is unique and so cannot itself be described in terms of a law. It does not follow that because the evolutionary sequence obeys the law of gravity, the law of natural selection and the Law of God that the result must be a mess.

'The idea that any concrete sequence or succession of events . . . can be described or explained by any one law, or by any definite set of laws, is simply mistaken.' The simplest refutation of Popper's argument lies in the facts. Consider the stars, not in their courses but in their sequences. Stars have their histories. Main sequence stars, of which the sun is a middle-aged member, *regularly* go through the same evolutionary development, eventually to reach the same end as collapsed dark bodies; and stars with other stateable initial qualities go through different, but predictable, phases to reach more or less spectacular ends. All this can be depicted on a Hertzsprung–Russell diagram and can be expressed as definite laws of succession.[1] It is clear that there is no question here of the law of succession of main sequence stars somehow riding roughshod over the laws of gravity, thermodynamics, *etc.* Given that there is a body of matter of a certain kind, with a certain density and volume, then gravitational forces will bring about its contraction which in turn will cause a rise in temperature which will then fire off a nuclear reaction and so on. With falling apples, the laws of nature work to produce a higgledy-piggledy succession of events; with stars they result in laws of succession.

This example is sufficient to refute Popper's general thesis. There are others which bear more directly on the possibility of laws of evolution.

---

[1] See, for example, E. G. Ebbighausen [1974], ch. 14.

The apple that Popper mentions, *Newton's* apple which in falling is pulled this way and that by the laws of nature, suppose we see it now as *Darwin's* apple. That is, suppose we see it in itself before it sets about its travels: in its *development*, which is rather more relevant to the question of laws of succession. Given a chance apple trees regularly produce apples and not plums or brussels sprouts. There are here regular sequences which, allowing for the usual vagaries of nature, can be caught in laws of succession. And in general, if Popper had looked beyond Newtonian objects he would have seen his thesis, that no sequence of three or more events can be described by a law, in Popperian fashion falsified in millions of instances. Organisms regularly proceed from zygote to adult. 'Few phenomena in the living world are so immediately evident as the begetting of like by like.'[1]

In so far as we are hypnotised by the example Popper uses to illustrate his argument, of Newton's apple battling its way to the ground, the question of laws of succession is begged. Falling apples, unlike stars and developing organisms, do not go in for such laws. The question is much more blatantly begged in his other argument. Here he is looking at the view of those who deny the evolutionary process is unique and whom he takes as holding a position that 'goes back to an idea of great antiquity—the idea that the life-cycle of birth, childhood, youth, maturity, old age and death applies not only to individual animals and plants but also to societies, races, and perhaps even to "the whole world" '[2] (p. 110). Popper does not deny that history or evolution may, in some respects, repeat itself but adds 'it is clear that all these instances of repetition involve circumstances which are vastly dissimilar, and which may exert an important influence upon further development. We have therefore no valid reason to expect of any apparent repetition of a historical development that it will *continue* to run parallel to its prototype' (pp. 110–11).

We might note, without making too much of it, that in considering this comparison of historical and evolutionary development with the life-cycles of organisms Popper is admitting that these life-cycles are *not* unique; and that he later forgets this when, as we have seen, he argues that 'no sequence of, say, three or more causally connected concrete events proceeds according to any single law of nature'. However, the more important point is that Popper's argument begs the question. He is, he tells us, going to refute those who believe the evolutionary sequence is not unique. But what he does is consider *one* such position, speedily disposes of it and leaves us to assume that this now vanquished alternative to his own view is the only one possible—or, at least, that it can stand in as a fair representative, or is the best, of all possible alternatives. If Popper were justified in this assumption, then biologists, more readily perhaps than historians, would agree with his sceptical conclusions for they have long been acquainted with Dollo's law—so-called—which in agreement with him states that evolution is irreversible. But there is, crying out for attention, a quite different way of denying that the evolutionary process is unique and this involves denying, first of all, that the evolutionary process on Earth is the only one. In short, we may point to the possibility, which many astronomers and biologists now think is a strong probability, that life has evolved on other worlds. To this it might be added that whenever life arises—or we might want to qualify this as 'wherever on earth-like planets life arises'—it always evolves through the same stages. If we tear our blinkered eyes from our planet and look to the heavens then there may be, for all we know, evolutionary laws of succession exemplified in a billion instances.

---

[1] Francois Jacob [1974], p. 1.

[2] Popper mentions Machiavelli, among others, as someone who holds such a cyclical view of history. Certainly Machiavelli thought that states arise from anarchy and eventually collapse back into it. In theory this should give rise to a new cycle but Machiavelli thought this was rarely realised. In conformity with the argument we have just examined, Machiavelli did not believe in one overriding law of development. Thus besides the law of historical cycles there is also, for Machiavelli, the law of the rapacity of neighbouring states (see his *Discourses on the First Decade of Titus Livius*, 1, 21).

Popper's more general argument against laws of succession—where all things are compared to apples falling on a windy day—will not rule out the possibility of laws of evolution. However, disposing of the general argument against the possibility of developmental laws does not entitle us to conclude that such laws describe the evolutionary sequence. After all, the evolutionary sequence of organic forms is quite different from the evolutionary sequence of stellar states or, even, the developmental sequences of individual organisms. For example, there does not seem to be some master plan that evolution unfolds as we can perhaps speak of the individual organism unfolding, as it grows, the plan incorporated in its genetic code. May we not then modify Popper's refutation of the cyclic theory of evolution and argue that these supposed flowerings of life throughout the universe will, in their different instances, 'involve circumstances which are vastly dissimilar and which may exert an important influence upon further developments'?

Suppose it is true, as some astronomers believe, that, barring accidents, any star similar to the sun will have its attendant planetary system, including an earth-like planet at a distance of about 93,000,000 miles. Suppose also that all, or nearly all, of these planets develop as the earth has done, resulting in the possession of chemically laden seas which constitute the warm 'soups' necessary for the origin of life. Even if life does emerge from all these soups on their separate earth-like planets, we still have no reason to believe that evolution will go the same way on each of them. There are bound to be *some* differences in the various soups, which may or may not be important for their evolutionary potential; and apart from this, evolution proceeds in a pretty chancy sort of way. It depends, at least on Earth, on natural selection acting on chance mutations. What mutations occur, and the environments that organisms adapt to, are bound to differ on the various planets so why should we expect to get elsewhere giraffes and lizards and frogs, for example, or, even, broad classes of animals corresponding in some degree to our own amphibians, reptiles and mammals?

This is a powerful argument but we should beware of adding to its power by taking too seriously an analogy Popper has developed between organic evolution and the history of ideas. Organisms, he claims, are like *theories*. 'The single organism is a kind of spearhead of the evolutionary sequence of organisms to which it belongs (its *phylum*): it is itself a tentative solution, probing into new environmental niches, choosing an environment and modifying it. It is thus related to its *phylum* almost exactly as the actions (behaviour) of the individual organism are related to this organism: the individual organism, and its behaviour are both trials, which maybe eliminated by error-elimination.'[1] I do not wish to deny that such an analogy has some advantages but if we push it too far then Popper's case against evolutionary laws is too easily made. For Popper holds that it is in principle impossible to predict the future course of knowledge[2]: if we look at the history of ideas, in itself without reference to the societies in which these ideas are thrown up, then we cannot find laws governing this history because the creation of new theories is an entirely free and in principle unpredictable activity. If organisms, themselves, are regarded as mere theories and they are not considered, also, in terms of, for example, their complex ecological relationships, then this is equivalent to treating evolution as a history of ideas completely divorced from a social context; and, of course, if we do this then it will be impossible to discover the laws controlling this continuous stream of organic ideas.

There are several issues which are relevant here. First, I suggest that, if we do wish to seek for evolutionary laws, and at the same time take seriously Popper's comparison of organisms with theories, then we should make the point that these living theories are trying to solve a limited number of problems only. Human theories, as uttered or written down, may be concerned with any of a number of things from science to theology to art and so on; theories in the form of more or less successfully living organisms are tied to the one overriding problem of survival. There is, then, that constraint at least on the evolution of living things.[1] We might go further and try to determine whether or not there is one fundamental problem (or perhaps a few fundamental problems) that lies behind the general problem of survival and which, in the ways it is solved, will have profound implications for the course of evolution. Such a problem, I think most biologists would say, is that of obtaining and disposing of energy.

---

[1] [1972], p. 243; see also p. 145.

[2] See above, p. 132, n. 1.

It may well be that the various possible modes of nutrition are of fundamental importance in understanding the way evolution has gone, a point well brought out by R. H. Whittaker's proposed new classification of terrestrial organisms.[2] Notice here that it might be the case that, leaving aside the industrial, *etc.* activity of creatures like man, the number of these possible modes is limited in the sense that there may be only a few broad kinds of way in which biochemical machines can obtain energy for nutritional purposes. This is not to say that organisms are not almost infinitely ingenious in the different ways they obtain energy but to suggest that these many ways of solving the problem are variations on but a few basic solutions—suggested by Whittaker to be photosynthesis, absorption and ingestion. Also, it might be that the various biological solutions to the problem of how energy is to be obtained can occur only in a more or less definite evolutionary sequence; which is determined partly by the fact that simpler solutions will generally be temporally prior to more complex solutions (as in historical development, man invented the club before the atomic bomb) and also that the biological solution of a certain stage of development will open up definite possibilities for succeeding stages. Thus it is widely held that the earliest organisms were heterotrophs living off the abundant materials in the primeval soup. Biochemically this early heterotrophism is much simpler than autotrophism which requires many more biochemical steps to obtain the same food material, so that in such an environment autotrophism would have been a disadvantage. However, as the soup became depleted, the advantages of autotrophism became more obvious. It is believed by many that the evolution of autotrophism as photosynthesis itself led to a change in the earth's atmosphere from a reducing to an oxidative one which had profound implications for the further evolution of the entire biosphere. These facts, if they be facts, taken in conjunction with, say, broad ecological considerations might lead to evolution, wherever it springs from similar circumstances, always proceeding in the same way.

[1] I do not wish to overlook the possibility that the great variety of theological, metaphysical, *etc.*, ideas might, considered as *ideology*, be found to represent a much smaller number of human interests.

[2] R. H. Whittaker [1969].

A second point is this. If there is life on other worlds we should not expect its evolution to proceed in every case down identical, twisting by-ways. However, there may be reasons for believing the highways are similar. In other words it may be a mistake to try to pin our laws of succession on to particular organisms rather than seeking some other, broader respect which might satisfy them. This suggestion is in direct opposition to Popper's 'methodological individualism'. At one point he tells us that 'the task of social theory is to construct and to analyse our sociological models carefully in descriptive or nominalist terms, that is to say, *in terms of individuals*, of their attitudes, expectations, relations, *etc.*—a postulate which may be called "methodological individualism" ' (p. 136). Such a methodology, whether applied to social theory or biology, is bound to fail to find general evolutionary laws and in this respect, like his undue emphasis on the history of ideas divorced from its general social context, turns our attention away from the possibility of such laws. Against Popper, then, I would urge that in attempting to formulate general evolutionary laws we do not mention individual animals, or even species, at all. The laws may well operate at a quite different level. They might, for example, include in their formulation reference not to Tibby or Rover, or even to dogs and cats in general, but to generalised energy systems and the ecological relationships between them.

Something like this is suggested by Whittaker's new classification. Instead of the older classification of living things into two main kingdoms, Plantae and Animalia, Whittaker suggests a five kingdom system (Monera, Protista, Plantae, Fungi, Animalia) which is based on three levels of organisation. The Monera are procaryotic and unicellular (first level), the Protista eucaryotic and unicellular (second level) and the Plantae, Fungi and Animalia all eucaryotic and multicellular (third level). What comes out also in Whittaker's classification are the major types of nutrition possible, photosynthesis, absorption and ingestion, which are ecologically related and also have profound implications for the types of organism that evolve. Thus with the rise of multicellular organisms the different modes of nutrition have led to quite different, although ecologically related, kingdoms with different ways of life. I am suggesting, then, that if we look at living things in the way depicted by Whittaker's classification—in terms of level of organisation and nutritive way of life—and also take account of general ecological relationships, then it is possible that we shall be able to frame general laws of succession.

I do not wish to suggest that these considerations weigh overwhelmingly in favour of the view that there are laws of evolution. There are, in fact, many unsolved difficulties. There certainly seems to be a kind of 'logic' about the way organisms have evolved on earth but, in reply to this, it might be urged that this 'logic' is evident in hindsight only. Even if we assume that evolution is caused by natural selection acting on chance mutations, how can we be sure that the same broad answers, in the shape of the kinds of organisms that these mutations give rise to, are everywhere arrived at?[1] It was mentioned earlier that when certain evolutionary stages are reached, these then allow further evolutionary possibilities. For example, the establishment of photosynthesis is thought to have led to a high concentration of atmospheric oxygen which made possible the further evolution of aerobically respiring organisms. But these are *possibilities* only; there is no guarantee that they will be taken up. We might guess that in some cases if, at certain stages of evolution, whole new ways of life present themselves as tempting possibilities, then some genotypes are bound to succumb to the temptations. Such a case, perhaps, was the colonization first by plants, then by animals, of the land; a supposition made a little more plausible by the fact that, in both plant and animal kingdoms, this colonization was attempted not once but several times. However, we may not always be as confident that such possibilities will be realised. May there not be times of evolutionary crisis, as it were, where general evolutionary advance depends on rare mutations occurring in unlikely combinations of circumstances? Such a time of crisis might have been at the point where the Protista evolved from the Monera if Professor Lyn Margulis's theory of the symbiotic origin of eucaryotic cells[2] is true. Another might have been involved in the origin of man. For anyone who believes in laws of evolution these questions must be answered. The point is, however, that they can be answered within science and the general question of whether or not there are laws of evolution is therefore itself answerable within science and is not to be determined on purely *a priori* philosophical grounds.

Macquarie University

REFERENCES

EBBIGHAUSEN, E. G. [1974]: *Introductory Astronomy*. Charles E. Merrill Publishing Co.
GRUNER, R. [1969]: 'Uniqueness in nature and history', *Philosophical Quarterly*, 19, pp. 145–46.
JACOB, F. [1974]: *The Logic of Living Systems*. Allen Lane.
MARGULIS, L. [1971]: 'Symbiosis and evolution', Scientific American, 225; reprinted in Edward O. Wilson (*ed.*), *Ecology, Evolution and Population Biology*, W. H. Freeman and Co.

---

[1] John Maynard Smith ([1972], p. 85) remarks 'we have as yet no grounds for asserting that if evolution has occurred elsewhere in the universe, it has done so by neo-Darwinist processes, although I would be willing to conjecture that it has'.

[2] Lyn Margulis [1971].

MAYNARD SMITH, J. [1972]: *On Evolution*. Edinburgh University Press.
POPPER, K. R. [1957]: *The Poverty of Historicism*. Routledge and Kegan Paul.
POPPER, K. R. [1972]: *Objective Knowledge: An Evolutionary Approach*. Oxford University Press.
SUCHTING, W. A. [1972]: 'Marx, Popper and historicism', *Inquiry*, **15**, p. 237.
WHITTAKER, R. H. [1969]: 'New Concepts of Kingdoms of Organisms', *Science*, **163**, pp. 150–60.

Brit. J. Phil. Sci. 29 (1978), 145–177 *Printed in Great Britain*

Copenhagenism and Popperism*

by YEHUDAH FREUNDLICH

Introduction
1 *States and Measurements*
2 *Ensembles and Probabilities*
3 *The Kochen–Specker Theorem*
4 *Successive Measurements and the Projection Postulate*
5 *Time Development and the Measurement Problem*
Summary

INTRODUCTION

There seems to be an unbridgeable gap between rival interpretations of quantum mechanics. One school speaks of the quantum state vector (and hence of the associated probabilities) as a property of individual quantum systems; another school insists that the quantum state vector is an ensemble property and that the associated probabilities refer to statistical distributions within the ensemble. The former school asserts that an observable of a system has a value in that system if and only if the appropriate observations have been made; the latter school claims that an observable always has a value in the system, though we may not always know it. The one school insists upon using quantum measurements for predictions only; the other for retrodictions also. The first school regards the discovery of the existence of non-commuting observables as placing restrictions upon the precision of our measurements (and hence upon the possible existence of a system with precisely defined values of the non-commuting observables); the second school maintains that these restrictions are of a statistical nature, limiting the precision not of our measurements of a system's observables, but only of our predictions about them.

Despite all this, I claim that the differences are fundamentally linguistic at least in this sense: (*i*) there exist two possible definitions of 'state' for an individual system—definitions which coalesce in a classical deterministic theory, but which must be differentiated in an indeterministic theory of

Received 20 *November* 1975

---

* I should like to thank Professor Jeffrey Bub for many interesting and enlightening discussions concerning quantum theory.

the quantum sort; (*ii*) on the basis of each of these two differing notions of state, one can construct a consistent conceptual scheme to describe quantum theory; and (*iii*) within each of these conceptual schemes, we find respectively the statements and attitudes attributed above to the two schools.

We have called these two conceptual schemes so constructed 'Copenhagenism' and 'Popperism'. I do not wish to get involved in a historical controversy concerning what Bohr and Heisenberg really meant, nor, of course, can I commit Professor Popper to the view bearing his name as presented here. My views certainly clash in some respects with those of some of the adherents of the Copenhagen interpretation, as well as with those of Professor Popper. Wigner in his [1967], for example, raises the issue of the effects of consciousness on wave-packet reduction in the Copenhagen interpretation; in my opinion, he has misconstrued the essentials of Copenhagenism. On the other hand, Popper in his [1967] concludes that observations are to be accorded no special status in quantum theory and that the reduction of the wave-packet is 'not an effect characteristic of quantum theory, but of probability theory in general' (p. 34); this again is, in my opinion, mistaken. For, I shall be concerned with showing that it is observation, not in the subjective sense of taking note of, but in the objective sense of interacting with a measuring instrument, which is and *must* be accorded a special role in quantum theory. (So that in my view, Popper's stress on the subjective–objective dichotomy as the basic issue between his and the Copenhagen interpretation is fundamentally misconceived.)

Despite these and other such disagreements between myself and some of the adherents of the Copenhagen interpretation on the one hand, and between myself and Popper on the other, I feel free to appropriate the names 'Copenhagenism' and 'Popperism' for my two schemes. For the afore-mentioned sets of claims do characterise respectively Popper's and the Copenhagen interpretation, and each set does follow from a particular choice between one of the two definitions of state we shall present. Choosing one definition, I find myself using Popper's interpretation, asserting that the quantum state vector is a property of ensembles, insisting upon a conceptual distinction between initial-state preparations and final-state measurements, and so on. Choosing the other definition, I tend to use the Copenhagen interpretation treating probabilities as properties of individual systems, using quantum measurements for predictions and not for retrodictions, and so forth. Thus I have no qualms in using the terms 'Copenhagenism' and 'Popperism' for my schemes.

In this paper I shall therefore be giving expositions of two interpretations of quantum theory, tracing clearly how, starting from two fundamentally different notions of state, each goes on to deal in its own, but nevertheless equivalent, way with measurements, the quantum state vector, ensembles and probability assignments, the projection postulate, the measurement problem and the time-development of the quantum state vector. Along the way, I shall treat such issues as the logical determinacy of future events, the 'peculiar' character of quantum indeterminism and its relation to the role of observations in quantum theory and the use of non-Boolean logics. Here I will confine myself to expositions of the two views and indicating their equivalence. In a second paper, I will deal with the specific objections of Popper and Park and Margenau to Copenhagenism. (Some of the basic notions expressed in this paper may be found in my [1977].)

Before proceeding, let me state at the outset that I shall take the difference between the two views exposited to lie in their choice of a conceptual scheme to describe the physics and *not* in the physics itself. One would therefore expect—indeed I shall develop the interpretations such that there are—no differing experimental predictions between the two views. That these two schemes can be so developed, that each can adequately handle all problems presented to it without enveloping itself in a great 'quantum muddle' is the thesis of this paper; I shall attempt to prove it by doing it.

1 STATES AND MEASUREMENTS

The textbook formulation (in both the literal and metaphorical sense) of what I shall call the Copenhagen notion of state is given by Jauch in his well known [1968] text. 'The state of a physical system,' he tells us, 'is the result of a preparation of a system. The state thus embodies the specific history which preceded the instant to which the state refers' (p. 91). And his formal definition of a state is given as follows: 'A state is the result of a series of physical manipulations on the system which constitutes the preparation of the state. Two states are identical if the relevant conditions in the preparation of the states are identical' (p. 92).

The weak point of Jauch's definition is obviously that it gives no account of the notion of relevancy. Jauch does say that conditions are relevant if they affect the system under consideration. But, he continues, there is no general account of which conditions affect the system because that is a matter to be decided empirically. Unfortunately, this is not enough. For a general account of relevancy, we need to know not *which* conditions affect the system (and thus *which* conditions are relevant) but what is *meant* by 'affecting the system' and thus what *constitutes* 'relevancy'. Yet, what

Jauch does not account for in the passage quoted here, he accounts for a few pages further on in his book. There he considers the mathematical determination of a state as a probability assignment on the (lattice) set of propositions (p. 94). Obviously, we are to take a condition as affecting the system if the probability assignment which the theory decrees the correct one, is affected by that condition in preparing the state.

A preparation is thus a way of guaranteeing that the past behaviour of the system should be of the sort from which we can make predictions in the context of the theory. Confining the discussion to quantum electron spin theory, the physical aspects of which are well known, I would say that, to the Copenhagenist, an electron is, in one sense, what has been done to it, its past behaviour. Of course, one doesn't really care in what manner the 'doing' occurred; we aren't really interested in the specific past history of the electron so long as the results are the same. But 'sameness of results' is *defined* by what the theory tells us we may legitimately expect.

This 'Copenhagen' notion of state leads one naturally to speak of identically prepared electrons as being in the same state, the state being attached to each individual electron. To be more precise, in the view I call Copenhagenism, one (formally!) considers the set of past spin-behaviours of individual electrons which are equivalent with respect to the legitimately[1] inferred future behaviour as *defining* a spin-state of the electron. The Copenhagenist assigns an attribute of z-axis spin-up to an individual electron and *means* to say by this that this electron is such that from its past behaviour one may legitimately (*i.e.* on the basis of the theory) infer that, should it be subjected to a z-axis spin-up filter, it will definitely pass that filter. This state may be said to be indeterminate as regards filters oriented in other directions in the sense that for a z-axis spin-up electron, we cannot legitimately infer whether it will or will not pass through these other filters. But it would be wrong in the Copenhagenist view to say that its *state* is indeterminate. For it has been stated; it is z-axis spin-up. It would definitely be wrong to attribute to it, say, an x-axis spin-right state, not in the sense that it would definitely not pass that filter, for it might pass it; but because the past behaviour of the electron is not such that we may legitimately infer that it definitely will pass through the x-axis spin-right filter.

It might be thought that the Copenhagenist is using non-Boolean logic when he says that the state of the electron is indeterminate as regards its passage through a particular filter. For Boolean logic decrees, so it might be argued, that the electron must either be in a state such that it will pass the given filter or it must be in a state such that it will not pass it.

---

[1] What constitutes a legitimate inference from past behaviour to future behaviour is decided by the physical theory under consideration.

But to argue so is to confuse two senses of the word 'state'. In the objection one equates state with future behaviour. Since the electron either will or will not pass the filter, we say it must be either in the state such that it will pass the filter or in the state such that it will not. To speak so is to use what I shall shortly call the Popperian notion of state. The Copenhagenist, howeve, is using the word 'state' to refer to *past behaviour* (as characterised by legitimate inferences concerning future behaviour). The Copenhagenist has merely said that the past behaviour of this electron is neither such that we may legitimately infer that this electron will pass the filter, nor is it such that we may legitimately infer that the electron will not pass the filter.

This statement tells us something about the lattice of propositions; for instance, that it might be non-Boolean. And such general statements, about what sort of future behaviour may be inferred from what sort of past behaviour, are themselves not propositions about individual electrons; they are not elements of the lattice. They are statements in the physical theory and tell us about the lattice. (Which is why quantum logic is not really a logic. In a logic the inference relation between two propositions $p \rightarrow q$ is itself the proposition $q \vee \neg p$. See Jauch and Piron [1970].) But when reasoning about such statements in themselves, we do not in any sense use a non-Boolean logic. A given electron is or is not any specified state whether we have observed (in the sense of taking note of) this state or not, where, of course, 'state' means what the Copenhagenist has taken it to mean.

An explicit formulation of what I call the Popperian notion of state (for an individual system) may be found in the works of general systems theorists (*e.g.* Mack [1971]). They define a system mathematically as a subset of the Cartesian product of an input and output set. (In the case of electron spin, the input set may be taken as the set of oriented spin-up filters and the output set being 'yes' or 'no'.) In general the output is *not* a function of the input; an input may not necessarily determine a unique output. A state representation of the system, however, is one in which a set of states is supplied so that for each given state the output *is* a function of the input. A state is thus a parameter making the output a function of the input.

But this assignment of state is merely a *formal* one. We can *always*, for any system whatsoever, find a state representation (see Mack *op. cit.*). For example, in the case of electron spin, we may consider the set of all possible assignments of 'yes' or 'no' to each oriented spin-filter as defining the state set and merely by assigning a parameter to each such truth 'valuation' assign a state to the system. ***There is no commitment here to the observability of the states, to our ability to determine the present state of the system on the basis of past inputs and outputs.*** It is the 'natural state' which is observable in this sense.

Mack defines a 'natural state' as a state (*i.e.* a function from the input set to the output set) determined by past input–output pairs. A natural state representation of a system exists, of course, only if the system is deterministic in the usual sense of the word, if past behaviour makes the upcoming output of the system a function of the upcoming input (in Mack's terminology, if the system is 'past-determined'). Thus, in quantum theory no 'natural state' representation exists in Mack's sense. Now, the Copenhagenist notion of state retains the idea of the state being defined by past behaviour. But since the past behaviour does not make the output a function of the input, we characterise the state by those inputs for which the output is uniquely determined (*e.g.* a *z*-axis spin-up filter is one whose ***past behaviour*** is such that if it were subjected to a *z*-axis spin-up filter, it would definitely pass it). The Popperian notion of state (***for the individual system***) retains Mack's original notion of state as a function from input to output defined over all possible relevant inputs, but forgoes the commitment to observability on the basis of past behaviour.

Thus, in the view I call Popperism, one considers (formally!) the set of truth 'valuations' over the set of possible 'questions' which may be asked of the system as defining the states of the system and any individual system is always taken (conceptually) as being in one such state. Therefore, when a Popperian says that a particular electron is a *z*-axis spin-up electron, he does not mean, as does the Copenhagenist, that this electron has passed a *z*-axis spin-up filter—though that may be a way of ***knowing*** that the electron is *z*-axis spin-up; he means rather that this electron is of the sort such that, if subjected to a *z*-axis spin-up filter, it will in fact pass that filter. We may sometimes know that this electron has this property, as when it has just emerged from a *z*-axis spin-up filter, and sometimes we may not know this, as when the electron has just emerged from a filter oriented along some other axis. If an electron which emerges from a filter oriented along another axis then passes this *z*-axis spin-up filter, we will then learn what was true of it all along, namely, that it also had this *z*-axis spin-up property.[1]

---

[1] Popper, I maintain, has implicitly assumed this notion of 'state' in his [1967]. There, he *argues* that the Heisenberg formula $\Delta p_x \Delta q_x > h$ applies to statistical distributions only. As for the individual particles we have $\delta p_x \Delta q_x \ll h$ (δ refers to our uncertainty about the individual particle, Δ refers to the scatter within the ensemble). Popper's *proof* is that for a particle passing a narrow slit (defining the Δq_x), there is 'hardly a limit to the precision' with which its momentum can be measured, say, in a spectrograph;

The Popperian is here not merely affirming the logical determinacy of contingent future events. Indeed, the Copenhagenist may also be quite willing to concede that a statement such as 'this electron will pass this x-axis spin-right filter in five minutes time' is always a logically determinate statement. He merely refuses to allow that it states a property of the electron. For the truth of the statement also depends upon whether we decide to *conduct* the experiment; and the statement is falsified if we do not conduct it. But to predicate a property of a system which stipulates its behaviour under appropriate conditions is not to make the property of the system contingent upon the existence of the appropriate conditions; only the factual behaviour to be inferred from the property is contingent upon those conditions. The Popperian is thus doing more than just affirming the logical determinacy of contingent future events. He is saying that the statement 'this electron has the property such that it would, if the experiment were to be conducted, pass on x-axis spin-right filter' is logically determinate and that its truth value is *independent* of whether the experiment is actually conducted, though, of course, we need to do this to *test* the statement's truth or falsity. He is thus committed to considering each electron as having assigned to it (conceptually) a truth value for each possible 'question' we may ask of it.

This does not, I reiterate, commit the Popperian to determinism. For an indeterministic theory is one in which systems with identical relevant past behaviour may behave differently in the future so that we cannot, within the framework of the theory, infer the future behaviour of an individual system on the basis of its past behaviour. The Popperian admits that quantum theory is indeterministic and that this indeterminism is quite an objective matter. He merely formulates indeterminism differently from the Copenhagenist. The Copenhagenist says that quantum theory is indeterministic because a system's present state (*i.e.* its past behaviour) does not determine uniquely its future behaviour. The Popperian states that quantum theory is indeterministic because a system's

---

its δp_x can be as small as we like. It is only Δp_x, which refers to the scatter of momentum in the ensemble, which will be large. (*ibid.* p. 20).

But Popper's formula makes sense only if δp_x and Δq_x refer to the particle at the same time. Popper assumes that the subsequent measurement of momentum *via* a spectrograph indicates that the particle had that momentum prior to the measurement, *e.g.* when passing through the slit. For Popper, this is merely a *formal* deduction. (It is not a contingent fact depending upon a particular physial theory.) If a particle shows up as having a certain momentum when we test for its momentum, we conclude that it had this momentum before testing for it. This may be done, I maintain, only if we adopt the Popperian notion of 'state'. The Copenhagenist would emphatically deny this claim (because he is using a different notion of 'state'). He would claim that the small δp_x is to be assigned to the particle only *after* it has passed the spectrograph (after which we no longer assign it the previous Δq_x).

preparation does not uniquely determine its present state (*i.e.* the truth valuation for each possible question that may be asked of the electron).

For the Copenhagenist then the state is past-directed in the sense that it tells us how the system was prepared (or what behaviour it has exhibited). For the Popperian the state of an individual system is future-directed in that it tells us how the system would behave if we were now to subject it to any experimental procedure.

These two different notions of state, though they coalesce in a deterministic theory, do not coalesce in quantum theory, and they lead to two quite different conceptual frameworks for describing quantum theory. To being with, for the Copenhagenist, an observable has a value in the system if and only if the appropriate measurements are made. (An electron is Copenhagenist spin-up if and only if the last spin-up filter the electron passed was a spin-up one.) Nor, in quantum theory, could *all* observables have definite values in the system at any specific time. (There are no experimental procedures which would make an electron, say, both Copenhagenist z-axis spin-up and Copenhagenist x-axis spin-right. In other words, there are no past behaviours which would allow us to predict on the basis of that behaviour *both* whether an electron would pass a spin-up filter if it should be subjected to one, *and* whether it would pass a spin-right filter if *instead* it were subjected to that filter.) For the Popperian, an observable always has a definite value in the system—by fiat!—though, of course, we may not know what the value is. Should we desire to know the value, however, we can always do so merely by testing for it. (If we desire to know whether an electron is Popperian spin-up— *i.e.* whether it would pass a spin-up filter if it were subjected to one—we merely subject the electron to a spin-up filter and see what happens.)

Further, the Copenhagenist and the Popperian have different views concerning the information conveyed by a quantum measurement. For the Copenhagenist, the results of the measurement indicate the state of the system subsequent to the measurement and, in general, not what the system's state was prior to this measurement. (Should an electron pass a spin-up filter, it is subsequently in a Copenhagenist spin-up state. That is, subsequent upon, but not in general prior to, the measurement, it is correct to say of the electron that its last spin measurement was a z-axis one and that the electron passed it spin-up. Nor can the result of this z-axis measurement convey information about the Copenhagenist state *prior* to this measurement; the result of a z-axis measurement cannot indicate to us along what axis the spin measurement prior to this one was made.) For the Popperian, the result conveys information about the state of the system both subsequent upon and prior to the measurement. (Should an electron pass a spin-up filter, we know that the electron is subsequently Popperian spin-up—*i.e.* should it again be subjected to that filter it would again pass it. We also know that the electron was Popperian spin-up prior to the measurement—*i.e.* that prior to the measurement it was the sort of electron that would pass a spin-up filter if it were subjected to one. Indeed, it *was* subjected to a spin-up filter, and the electron *did* in fact pass that filter.)

2 ENSEMBLES AND PROBABILITIES

So far, I have virtually ignored the statistical aspects of quantum theory. I must now consider how the Copenhagenist and Popperian respectively describe ensembles and assign probabilities within their respective conceptual frameworks.

For the Copenhagenist, the electron's individual state is fixed by its past behaviour. An ensemble of identically prepared electrons then for him consists of an ensemble of electrons with identical individual states. Each electron so prepared is in that particular state.

It might be objected that it is not enough for the electrons to have undergone individually identical past behaviour for us to consider this past behaviour as defining their individual states. The past behaviour, we have said, is characterised by what we may legitimately infer concerning future behaviour. But if our inferences are probabilistic, they are inferences about ensembles and not about individual electrons. Thus it would seem that though an individual electron does exhibit past behaviour, it is not of the sort which allows for legitimate inferences about the behaviour of the individual electron.

If this were the situation, if indeed among the set of relevant questions about which the theory is in general indeterministic, there were no legitimate inferences for the individual electron, we would have a classical indeterministic theory. Such a situation might be envisaged, if we consider a theory of die-casting as being 'irreducibly' indeterministic. Different properties of the die reflecting, say, its homogeneity or non-homogeneity, or, say, its past statistical behaviour, might lead to different probability assignments. But we could not in general predict anything for the individual throw of the die. This is not the situation, however, in quantum theory. Given a particular preparation procedure for an ensemble, there are legitimate inferences about the individual electrons. (In our spin case, in an ensemble of electrons which has passed a particular filter, we may legitimately infer for each and every electron that, if subjected again to that filter, it will again pass it.[1]) Thus a preparation procedure characterises a definite individual state. Further, this characterisation of the individual electron (as being the sort prepared in such a way that it may be legitimately inferred that it will pass a z-axis spin-up filter) suffices to determine, by virtue of quantum theory, the statistical behaviour as regards ***all other*** spin-filters to which the electron may be subjected. That is, ***the individual state characterises the statistical distribution in the ensemble and the ensemble preparation procedure.***

Let us consider again this important point about the special nature of quantum indeterminism. For it is certainly not the mere use of statistics which characterises quantum theory, nor is it its 'irreducible' indeterminism. It is rather a particular type of indeterminism which both separates quantum theory from other indeterministic theories and, as I shall assert later in this paper, generates the conceptual problems of quantum-measurement theory.

In electron spin theory, each vector state—each pure state—consists of a probability assignment over the set of oriented spin-filters. In each such state there is one and only one oriented spin-filter which is assigned the probability 'one'. Further, to each oriented spin-filter there is one and only one pure state which assigns that filter probability 'one'. Finally, a preparation procedure for a given pure state consists merely of passing through that oriented filter for which the *desired* pure state assigns probability 'one'. This is true for quantum theory in general. There is an isomorphism between the 'atomic' questions which may be asked of the system and the pure states as probability assignments on the set of questions, such that if p is the atomic question and α_p the pure state corresponding to it, $\alpha_p(p) = 1$. (See Beltrametti and Cassinelli [1972]). (Within the Hilbert space formulation of quantum theory, this is the correspondence between one-dimensional projectors and the pure states projected out by the projectors.) Finally, the physical preparation procedures for the pure states consist merely in answering 'atomic' questions.[2]

---

[1] This is merely the phenomenon of interference. A coherent superposition of pure states allows for interference between those states and thus for predictions for the individual system. As for the double slit experiment, Aharonov *et al.* in their [1969] have constructed an operator for which this particular state is an eigenstate. They have also considered (in their [1970]) the more general question of finding for any particular experiment the operator for which a given pure state will be an eigenstate of that operator (as opposed to the typical problem in quantum mechanics of finding the eigenstate for a given operator). This is trivial for the spin one-half particle. Given any two component spiner, σ, we can always find a direction $\hat{n}$, such that $\sigma . \hat{n}$ is sharp.

[2] We have stressed that the peculiarities of quantum theory arise from these very points. It would therefore be appropriate to show how our characterisation of the peculiarities of quantum theory implies, what has been suggested as characterising the theory, namely, the non-Boolean nature of its propositional lattice (see Jauch [1968]).

All this contrasts sharply with, say, a statistical die-theory. Probability assignments do not lead, in general, to any predictions for the next individual throw of the die. Nor does selecting an ensemble of dies, all of which have shown a particular face on their last throw, constitute a preparation procedure leading to a particular statistical distribution for the next throw.

The Copenhagenist incorporates these peculiarities of quantum indeterminism in his very language. He exploits the isomorphism between measurements as an individual system's way of answering questions (*i.e.* as a final-state measurement) and measurements as a way of preparing ensembles (*i.e.* as initial-state measurements); he exploits it so as to speak neither of questions nor of preparation procedures, but only of measurements. The Copenhagenist draws the preparation procedure into the description of the electron's individual state (as its past behaviour); then, since answering a question defines a preparation procedure, an electron by answering a question changes its state.

For the Popperian, the present individual state of the electron refers to future behaviour. It tells us what filters the electron will in fact pass should the electron be subjected to them, and not, as the Copenhagenist would have it, that the electron's past behaviour is such that we could infer from it that the electron would pass those filters. Since identically prepared electrons do in fact afterward behave differently, the Popperian concludes that identically prepared electrons may be in different states. In particular, an ensemble of electrons which has passed a z-axis spin-up filter (and thus an ensemble of identically prepared electrons) consists literally and classically of electrons half of which are x-axis spin-right and half of which are spin-left.

But, the Popperian admits, there is something about electrons and spin-filter apparatuses which always gives rise to this identical statistical

---

In such a lattice, the partial ordering relation $<$ is taken to mean the following: two propositions p, q are related via $p < q$ if and only if whenever p (*i.e.* when the answer to the equivalence class of questions defined by the proposition p will be 'yes'), then also q. The mathematical states are probability assignments on the propositional lattice and must obey certain additivity conditions expected of probabilities. In particular, it follows from these properties, that for any state α we have $\alpha(p') = 1 - \alpha(p)$, where p' is the orthocomplement of p, and if $p < q$ and $\alpha(p) = 1$, then also $\alpha(q) = 1$.

The peculiarity of quantum theory, we have suggested, is that to each atomic proposition p there corresponds a statistical (or mathematical) pure state α_p such that $\alpha_p(p) = 1$. Now, in a Boolean lattice, for any atomic element p and any element x, either $p < x$ or $p < x'$, but not both. This follows from the more general theorem that in a Boolean lattice, either x or x', but not both, is contained in any ultra-filter (Abbot [1969], p. 197). In a Boolean lattice, therefore, the state α_p corresponding to the atomic proposition p, and thus for which $\alpha_p(p) = 1$, is two valued over the entire lattice. The existence of *genuine* statistical states corresponding to atomic propositions, thus implies the non-Boolean nature of the lattice.

distribution, something objective, something 'kickable', as Popper puts it in his [1967]. The Popperian is forced to say that it is a property of electrons, *qua* electrons, and *z*-axis spin-up filters, *qua* themselves, that whenever an ensemble of electrons passes such a filter it gives rise to this particular distribution of states as defined by the set of all oriented spin-filters. This is a property of the entire experimental set-up, its 'propensity' to give rise to a particular statistical distribution. And as such it should be taken to be a 'real physical property of the single experiment, or, more precisely, of the experimental conditions laid down by the rule that defines the conditions for the (virtual) repetition of the experiment' (*ibid.* p. 33).

The quantum state vector (the *psi*-function) is thus, for the Popperian, an *ensemble* property. Since quantum theory is quite deterministic about ensembles (ensemble preparations uniquely determine ensemble behaviour), the Popperian and Copenhagen notions of state, *when applied to ensembles*, coalesce. The Copenhagenist and the Popperian, of course, do not at all agree about the status of the quantum state vector. For the Popperian, it is an ensemble property, whereas, for the Copenhagenist, as I have said, it is the property of an individual system. But, the Popperian—who, after all, does take the quantum state vector to be an ensemble property—may, because of quantum theory's determinism about ensembles, speak of the state vector as referring *either* to the preparation procedure the ensemble has undergone (the *ensemble's* past behaviour) *or* to the statistical distribution to which the procedure gives rise (the *ensemble's* future behaviour), since it is an objective fact that particular preparation procedures do give rise to particular statistical distributions.

The Popperian thus separates (conceptually!) the intimate connection between behaviour and that which may be inferred from it; between experimental procedures which 'question' electrons and experimental procedures which prepare ensembles with particular statistical distributions. He ignores (again conceptually) the fact that in quantum theory these procedures are identical. Considered as questions (*i.e.* as final-state measurements), experimental procedures define the individual states; considered as preparation procedures (*i.e.* as initial-state measurements), they define the ensemble states.

And thus for the Popperian, quantum-mechanical probabilities are in one sense objective and in another sense subjective. They are objective in the sense that there are 'propensities' for particular preparation procedures to give rise to particular statistical distributions. (Individual measurements have no effect, of course, upon these 'propensities'; the objective Popperian probabilities do not change.) The probabilities are subjective in the sense that each individual system is in fact in a Popperian state at any time. (An electron emerging from a spin-up filter already has a truth valuation concerning its passage through a spin-right filter.) We, however, not knowing the state, assign a subjective probability to the system in accordance with the statistical distribution in the ensemble to which the system is associated. It is this subjective probability which changes upon measurement because of a shift in the reference class with respect to which the probabilities are calculated. (The electron passing a spin-right filter is no longer to be considered as part of the ensemble it was before—the ensemble passing a spin-up filter—but of an ensemble passing a spin-right filter.)

The historical Popper has made the claim that this is all there is to wave-packet reduction, that there is nothing in the quantum version of the projection postulate other than a mere shift in the reference class. This, I maintain, is wrong. But, so long as we confine ourselves to a one-measurement experiment in electron spin theory, the special nature of quantum indeterminism will not be apparent within the Popperian framework. To appreciate the 'peculiarities' of quantum theory, we shall have to consider either quantum theories described in a Hilbert space of dimension greater than two (such as the massive spin-one particle theory, or the free particle theory), or the case of successive spin measurements. I shall consider both of these alternatives in the following two sections.

3 THE KOCHEN–SPECKER THEOREM

The Popperian defines the state of an individual system as a two-valued truth 'valuation' over the set of questions which may be asked of the individual system. The following examples cause us to question whether such an assignment of truth values is consistent with quantum mechanics.

Consider first the case of a massive spin-one particle as treated by Bell in his [1966]. As is well known, for the massive spin-one particle, the squares of the magnitudes of the spin components along any orthogonal triplet of axes are 'compatible' observables which can be simultaneously measured; and two of these components will always have magnitude 'one', and one of them 'zero'. Now, Bell shows that one cannot assign spin-magnitude values of 'one' or 'zero' to each direction in space such that for any orthogonal triplet of axes, the particle will have a spin magnitude of 'one' along two of those axes and 'zero' for the third. One cannot therefore consider, even conceptually, each particle as having a definite spin magnitude along each direction of space. This apparently disproves the Popperian interpretation.

Another example has been offered by Aharonov (in a private communication). The spectrum for the observables X and P_x of an unbounded particle are continuous. On the other hand, the spectrum of the operator $X^2+P_x^2$ is confined to odd integral values, as can be readily proved by the algebraic methods used in the calculation of the energy levels of a harmonic oscillator. Consider now an ensemble of particles prepared with a definite x-value and then subject them to a momentum measurement. The Popperian takes each individual particle to have had, in the interval between the preparation and the momentum measurement, both a definite x-value (the common one as prepared) and a definite p-value (the particular one which it shows up with in the momentum measurement). If, in addition, he also takes each particle to have a definite $X^2+P_x^2$ value, he seemingly runs into a contradiction, because particles in the above ensemble will have assigned to them (x, p) pairs such that $X^2+P_x^2$ will not be an odd integral. Yet all the particles in the ensemble must have such values for the corresponding operator.

The Popperian escapes these dilemmas by appealing to his distinction between measurements as experimental arrangements giving rise to results, and as preparation procedures. In the case of the massive spin-one particle, for example, the argument assumes that the Popperian assigns a definite spin magnitude to each direction of space independently of how that magnitude is measured. But the Popperian need not so assign magnitudes, as he distinguishes between the different tests for measuring a magnitude and considers them independent properties. In other words, a particle may be of the sort that it will show a spin magnitude of 'one' along the x-axis if this should be measured simultaneously with the spin magnitudes along the y and z axes, yet would show an x-spin magnitude of 'zero', were it to be measured simultaneously with the spin magnitudes along, say, the u, v axes. Again, the Popperian may logically assign properties in this way because these properties refer to behaviour elicited in different experimental set-ups.

One may then ask, in what sense is the x-spin magnitude an observable? That is, what is there in common between the x-spin magnitude when measured with the y- and z-spin-magnitudes and the x-spin magnitude when measured with the u and v ones? The Popperian answer is that there is an operator identity here ***which relates to distributions of properties in ensembles, that is, as a preparation procedure.*** In any prepared ensemble, *i.e.* one in a given pure (statistical) state, the proportion of particles with a given x-spin magnitude is independent of which other mutually compatible spin magnitudes are measured along with it. However, this does not imply that a unique x-spin magnitude must be assigned to each individual particle independently of what other magnitudes are being tested for along with it.

Similar remarks may be made for Aharonov's examples. The Popperian considers each particle to have assigned to it a value for any type measurement that can be made upon it, in the sense that if this measurement is made, the particle will show the assigned value. But the value assigned to an $X^2+P_x^2$ measurement is not the sum of the squares of the values assigned to the X and P measurements. And if again the question is raised as to what the functional relationship between the observables expresses, the Popperian would again reply that it is an operator relationship relating to distributions in ensembles. The proportion of particles which will show a particular value for the $X^2+P_x^2$ measurement in an ensemble in any pure state is obtained by calculating the square of the magnitude of the projection of that state upon the eigenstate of the operator $X^2+P_x^2$. Or, equivalently, by Gleason's theorem (see his [1957]), an ensemble of particles prepared by having been subjected to an $X^2+P_x^2$ apparatus and having shown a definite value, is to be described in Hilbert space as an eigenstate of the X^2+P_x operator with that eigenvalue. In the Popperian interpretation this is taken to mean merely that the proportion of particles in that ensemble having a definite value assigned to them for any specific measurement is to be calculated according to the usual quantum-mechanical rules with our ensemble described as given above.

These examples *do* show that the Popperian, in contrast to the Copenhagenist, cannot accept the functional relationship between operators as expressing a relationship between observables for individual systems. The Kochen–Specker Theorem (see Kochen and Specker [1967]) states this as a general rule for systems described in a Hilbert space of dimensions greater than 2. If the set of questions which can be asked of the system is identified with the projections in the Hilbert space, then in a Hilbert space of dimensions greater than 2, a consistent truth evaluation cannot be assigned to this set of questions while maintaining the functional relationship of the operators also for the individual observables. This result is important as it rules out this type of 'hidden variables' theory of quantum mechanics. It does not, however, rule out Popperism, as the Popperian, while maintaining the functional relationship of operators, abandons this relationship for individual observables *so as to retain the usual predictions of quantum mechanics*. Here is one opportunity for developing a rival theory of quantum mechanics, within the framework of Popperism, say, by retaining some of these relationships for individual observables. This, the Kochen–Specker Theorem implies, would lead to predictions differing from those of quantum mechanics. But, as stated in the introduction, this would carry us beyond the scope of this paper.

It is perhaps appropriate here to compare my defence of the Popperian viewpoint with Gardner's [1972] defence of Popper. (My purpose, however, is not to argue against Gardner, and certainly not to argue for a 'correct' interpretation of Popper, but to clarify my Popperian position by contrasting it with another.)

Gardner defends Popper's claim that observables always have values, even when not measured, against the objections raised by the Kochen–Specker Theorem, by concluding that measurements need not yield the values which observables had prior to measurement. Gardner distinguishes between 'quantitative properties' and 'observables'. According to him, 'when a quantitative property is measured, its value always coincides with that of the corresponding observable. However, at other times, the quantitative properties may have values which they never have when measured because the algebraic structures of the observables prevents the corresponding observables from having these values' (p. 19).

I am unsympathetic to Gardner's 'unmeasured quantitative properties'. These 'unmeasured properties' are completely detached from experience; not only have they not been observed, but, not having been observed, they are now unobservable *in principle*. There exists no way of testing for these properties (because the values yielded by a measurement are totally disconnected from the values prior to the measurement). I prefer Popper's statement in his *Logic of Scientific Discovery* (p. 424), that a property has irreducible dispositional components, that a property must indicate something about what would happen in at least some circumstances; Gardner's unobserved properties do not.

In what I have called the Popperian position, properties have irreducible dispositional components, and measurements serve as tests for the properties in question. The Kochen–Specker dilemma is resolved, not by severing the connection between the values of the properties and the values yielded in measurements, but by denying the existence of the respective properties. Not that, say, the value of the spin-x magnitude is indeterminate when not measured; rather, there simply is no such thing as a Popperian spin-x magnitude property for a massive spin-one particle in the sense that this value would be yielded by measurement regardless of what other simultaneous measurements were to be made along with the spin-x one. A massive spin-one particle *does* have a Popperian property in virtue of which, if the appropriate measurement were to be made, a particular triplet of numbers would be yielded for a particular triplet of orthogonal directions. But the Kochen–Specker Theorem indicates that this property is *not* the logical conjunction of three properties in virtue of which each of these three numbers would be yielded. Thus a massive spin-one particle does not have a Popperian property in virtue of which it yields a particular spin-magnitude value for the x-direction in experiments where other directions are measured along with it.

Now, I too admit that in a certain sense there is a spin-x magnitude which is observable. I too distinguish between 'observables' and 'properties' and, again like Gardner, I find two such distinctions: On the one hand, a familiar, non quantum-mechanical one, and, on the other hand, a "peculiar" quantum-mechanical one. The first distinction has to do with the mere recognition that different observations made under differing circumstances may indicate the same thing about a system. One may test for a Popperian z-axis property either by passage through a z-axis spin-up filter, or, equivalently, by passage through a device which rotates spin by 90° and then by passage through an x-axis spin-filter. What makes them equivalent is the theory. That is, according to the theory, if one behaviour were to be elicited in the first experimental arrangement, then, in principle, the other behaviour would be elicited in the second. Thus, in a very definite sense, a descriptive property may be viewed as an equivalence class of dispositional components. If we are interested in the equivalence class as a whole (*i.e.* the descriptive property being tested for), then we usually describe our observations in terms of the equivalence class ('I observed that the electron-was z-axis spin-up'). If we are interested in the particular manner in which the observations took place, *i.e.* the particular dispositional component which indicates the property, then we distinguish between what is observed and what the observation indicates. When understood in this sense, I agree with Gardner that there is nothing strange about the fact that the results of measurements depend upon the entire experimental arrangement designed to elicit the result.

But the Hilbert-space operators in quantum theory correspond to observables which, in my version of the Popperian position, must be distinguished from properties in a special and unique sense. I am not quite sure about Gardner's position. According to my version of the Popperian position, however, a Hilbert-space operator corresponds to a way of preparing *ensembles* and *not* to something about an individual system. Of course, ensembles are prepared by observing something about the individuals composing the ensemble, but these observations are used to make (in general) statistical predictions about the ensemble. In this sense there is a spin-x magnitude observable which determines statistical distributions within ensembles prepared in this definite way and the distributions (which the spin-x magnitude observable determines) are independent of what other compatible observations have or will be made. For my version of the Popperian position then, the fundamental quantum-mechanical distinction is between the use of an observation as a *test for an individual property* and its *use as a preparation procedure* for ensembles leading to particular distributions in ensembles.

4 SUCCESSIVE MEASUREMENTS AND THE PROJECTION POSTULATE

I have stressed above that for my Popperian, as for the historical Popper, the objective probabilities of quantum theory only change in the trivial sense that there is a shift in reference class. The historical Popper has claimed that the projection postulate in quantum theory is completely analogous to the change in probability which occurs in any probability theory when the reference class is shifted, *i.e.* the change from $p(B)$, the probability for event B, to $p(B/A)$, the probability of B given A. To exhibit the uniqueness of the quantum-mechanical projection postulate, I shall consider successive spin-measurements.

Let us consider, for definiteness, the spin analogue of that famous question, 'How can the slit through which the electron does not go affect the electron?'. Figure 1 is a schematic diagram of the experiment I want to consider. On the left is an x-axis spin-right filter which selects electrons from a beam. A Stern–Gerlach apparatus oriented along the z-axis separates the spin-up and spin-down components of the beam which now travel along the (symmetric) upper and lower channels respectively. Electric fields guide the beams along the channels and recombine the two component beams, where they enter an x-axis spin right filter. If no instrument measures the z-axis spin of the electron then the beams combine coherently and pass the x-axis spin-right filter on the right. Any measurement of the z-axis spin, say, by placing a Geiger-counter in one of the channels (and this can be done even after the beam has entered the channel) destroys the coherence, and the beam intensity passing through the final filter will thus be reduced to one-half of its original density. Let us, to be specific, place a Geiger-counter in the upper channel and consider the case of one electron which, having passed the filter on the left, enters the S–G apparatus, does *not* trigger the Geiger-counter in the upper channel, and does *not* pass the filter on the right. The, by now classical,

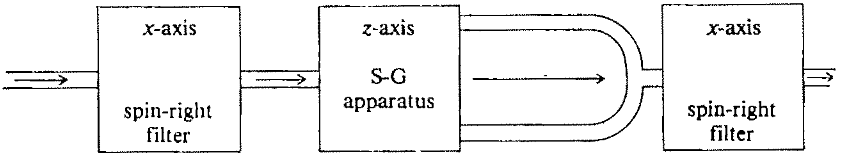


Fig. 1. *A schematic diagram of a successive spin measurement experiment.*

question is: if the electron passed through the lower channel, how does a Geiger-counter placed in a channel through which the electron does not pass, affect the electron spin so that it is no longer spin-right?

In his [1967] paper, Popper answers this question in its classic form in the double-slit experiment, 'How does the electron know whether the other slit is open or closed?' by appealing to Landé's arguments about periodicity conditions. 'It is the screen . . . which "knows" whether there is a periodicity Δq_x built into it or not and which therefore "knows" whether it can absorb momentum packets of the size $\Delta p_x = h/\Delta q_x$. The particle does not need to "know" anything: it simply interacts with the screen (which "knows") according to the laws of conservation of momentum *and* of space periodicity; or more precisely, it interacts with the total experimental arrangements . . .' (*ibid.* p. 24). Thus Popper appeals to the fact that the experimental conditions are different when one or two slits are open.

I do not quite see the force of Landé's arguments in Feynman's version of this experiment where instead of closing or opening one of the slits we place or do not place a Geiger-counter in front of one of the slits (see Feynman [1965] pp. 1–6 to 1–9). It would seem *prima facie* that placing a Geiger-counter in front of one slit does not change the total condition of the screen and should not affect the possible momentum exchanges between the screen and the electron passing through the other slit. Similarly in our experiment we would wonder as to how the Geiger-counter in the upper channel alters the entire apparatus so as to affect the electron in the lower channel.

Perhaps a brilliant physicist like Landé may come up with some scheme to explain this. But, for our purposes, that would really be *uninteresting*. For a simple man, such as myself, need not investigate the physical peculiarities of the experimental conditions for each possible test. He has a sure-fire method for discovering what would alter, in a relevant way, the 'total experimental conditions'. Namely, *anything which tests a relevant aspect of the electron behaviour alters the total experimental conditions, whether it be by closing one of the channels or by placing a Geiger-counter in front of one of them, or by any other means whatsoever which tests whether the electron spin is up or down.* What is needed is a *conceptual* scheme which takes account of the fact that if no test of z-axis spin is made, if the Geiger-counter is not placed in any channel, then any electron would definitely pass the final filter on the right, whereas, when a test is made, any test, one electron might not pass the filter.

The Popperian (Popperian$_\mathrm{I}$) may choose to describe the experiment thus. The electron, when it emerged from the filter on the left, had an x-axis spin-right value, and we knew this, ***and*** a z-axis spin-down value, but we did not know this. Placing a Geiger-counter in the upper channel is a way of testing (or, of learning) its z-axis spin-value. But in doing so, we have, at least for our electron, altered its physical Popperian state. It is no longer both spin-right and spin-down, it is now both spin-left (though we cannot know this until we have tested this with the filter on the extreme right) and spin-down (which, of course, we know now). And in any ensemble of such electrons and *any* such test of the z-axis value, the states of some electrons will be altered, the redistribution of states being *independent* of what constitutes the test for the z-axis spin value and being that as defined by the usual projection postulate of quantum theory. If the Popperian opts for this description of the dual experiment, then this randomisation procedure (for any test whatsoever) is his equivalent of the Copenhagenist's projection postulate. And there is nothing like it in classical probability theory.

On the other hand, the Popperian (Popperian$_{\text{II}}$) may claim that the insertion of the Geiger-counter in the upper channel does not alter the state of the electron; rather the question asked of the electron has been changed.[1] And any test *whatsoever* of whether the electron went through the lower channel constitutes a new question. Thus the electron is to be described as entering the lower channel and in a state such that it would pass an x-axis spin-right filter if no test is made of its travel through the lower channel and such that it would ***not*** pass the filter if the test is made.

Now one cannot logically assign a truth value 'true' to the statement that a system will exhibit behaviour A, and a value 'false' to the statement that it will exhibit this behaviour if it is tested. For then, by definition, the test is not a test of whether or not the system exhibits behaviour A. And indeed the Popperian does not do this. He insists, as logically he must, that, for any electron, identical truth values be assigned to both statements, that the electron travels through the lower channel and that if tested it will show this. For Popperian$_{\text{II}}$, however, if behaviour A is one component of two successive behaviours A and B, then the observable O_1, which consists of behaviour A followed by behaviour B, is different from observable O_2, which consists of behaviour A *as tested* followed by behaviour B, in the sense that identical truth values need not be assigned to claims that the system behaves in the corresponding manners.[2] If we incorporate this into the general rule, as required by quantum theory, that for any given preparation procedure, though the probability for an electron in the ensemble to behave as A followed by B is simply $p(A)p(B)$, the probability of behaving as A tested followed by B is $p(A)p(B/A)$, where the conditional probability $p(B/A)$ is given by the usual quantum rules, then this general rule becomes, for Popperian$_{\text{II}}$, the equivalent of the projection postulate. And again such a rule is unique to quantum theory.

---

[1] I am not really attributing opposing views to the two Popperians. The Popperian would rightly insist that whether he chooses the first or second of the two alternatives outlined in the text depends upon the 'rule that defines the conditions for the (virtual) repetition of the experiment'.

[2] This makes behaviour corresponding to O_1 untestable even in principle, *i.e.* by definitio For any test of 'A followed by B' must be made using experimental arrangements whic test for A.

5 TIME DEVELOPMENT AND THE MEASUREMENT PROBLEM

We come now to the 'gut' problem for all would-be interpreters of quantum theory, the measurement problem. The problem is usually presented to the Copenhagenist by pointing out to him the obvious disparity between the two types of changes in states, that which follows from a Schrödinger-type time development when the system interacts with other systems (Process II of von Neumann [1955]) and that which follows from measurement as dictated by the projection postulate (Process I of von Neumann [1955]). The latter type change is not unitary and is not deterministic, whereas the former is. Yet the effect of an interaction with a macroscopic measuring device upon the state of a system should, *prima-facie*, be merely some suitable 'macroscopic limit' of microscopic type interactions and thus itself be describable by a Schrödinger-type development. But this doesn't seem possible.

I have insisted elsewhere ([1972]) that, for the Copenhagenist, there are two interdependent aspects of the problem, a physical one and a conceptual one, related to the two stages in which the state vector is reduced. In measurement, a coherent superposition of sharp states is reduced to one of those states. In the first stage, therefore, a pure state, considered as a coherent superposition of sharp (pure) states, evolves into an *incoherent* superposition of those states. In the second stage, we, using pencil and paper, simply cross out the unobserved branch of this incoherent superposition.

Each stage is necessary. The first because the removal of a branch from the state description implies that interference between the remaining and removed branches is no longer possible even in principle (*i.e.* that the superposition of these two branches is an incoherent one); the second, because a measuring process *is* a process in which the state is reduced to one of the sharp states defined by the measuring device. For the Copenhagenist, the combined system of measured system and measuring device is not in some limbo world defined by an incoherent superposition of states, but is *really* in a definite state: the observed one, or if we haven't bothered to look, the one we would find if we were to look at the measuring device.

With each stage of this process there is a corresponding problem. First, there is the problem of reconciling the development, in the measuring process, of a pure state into an incoherent superposition of pure states with the fundamental Schrödinger-type time development which seemingly does not allow for such a development. I have referred to this problem (*op. cit.*) as the physical aspect of the measurement problem.[1] Second, we need to

---

[1] The development of the state vector after measurement into an incoherent superposition of states (when the particular result of the measurement is ignored) cannot be explained by simply appealing to the correlation between the measuring device and the original system, which, if ignored, makes the state of the original system incoherent. For the problem of incoherence remains even if we take into account the *entire* system, measured and measuring systems together.

More explicitly, let the interaction of the systems be such that

$$|s_k\rangle\,|M_0\rangle \to |s_k\rangle\,|M_k\rangle, \tag{1}$$

where the $|s_k\rangle$ are a complete set of orthogonal (pure) states of the measured system S as defined by the measuring device M, where $|M_0\rangle$ is the initial state of M, and $|M_k\rangle$ the states of M correlated with the sharp states of the measured system S. Let now

$$|\psi\rangle = \Sigma\, c_k\,|s_k\rangle \tag{2}$$

represent a (pure) state of S, so that from (1) and the linearity principle,

$$|\psi\rangle\,|M_0\rangle \to \Sigma\, c_k\,|s_k\rangle\,|M_k\rangle. \tag{3}$$

One may argue that $|\psi\rangle$ has 'evolved' into an incoherent state, for by ignoring the indeterminate phase factor multiplying each branch of $|\psi\rangle$, no interference can be affected between the various branches.

But to argue thus is to evade the problem. Why is the phase factor introduced by the measuring system indeterminate ***even in principle***? Why can we not, taking the right hand side of (3) as the state description of the combined system. consider them as a pure state, and design an appropriate interference experiment? Indeed, if M were a microscopic system, we can. We can, for example, at least in principle, time-reverse (3) (still ignoring the particular [illegible] for M) and then cause interference between the various $|s_k\rangle$. The claim that [illegible] and side of (3) is not a pure state is to claim that we cannot, using the products [illegible] alone, get back to the left hand side of (3).

More generally, [illegible] side of (3) is a pure state and we let it evolve (due to any sort of interaction), so that

$$|s_k\rangle\,|M_k\rangle \to |s_k(t)\rangle\,|M_k(t)\rangle, \tag{4}$$

then, in an ensemble of S's and M's, the probability of finding S in some state $|t_j\rangle$ and M in some state $|N_1\rangle$ is

$$p(t_j, N_1) = |\Sigma\, c_k \langle t_j \mid s_k\rangle \langle N_1 \mid M_k\rangle|^2. \tag{5}$$

On the other hand, if the right hand side of (3) is incoherent, then instead of (5), the probability will be

$$p(t_j, N_1) = \Sigma\, |c_k \langle t_j \mid s_k\rangle \langle N_1 \mid M_k\rangle|^2. \tag{5$'$}$$

Now, (5) and (5′) are compatible if and only if the sums in them reduce to a single term, *i.e.* no state $|t_j\rangle\,|N_1\rangle$ can be found which has a projection on more than one of the states $|s_k(t)\rangle\,|M_k(t)\rangle$, for any t in any interaction whatsoever. Since S, by hypothesis, does have states which are not orthogonal to all the $|s_k\rangle$ except one (*e.g.* $|\psi\rangle$), this implies, in particular and as mentioned above, that the time reversal of (3) cannot be effected. (Note, if it could, then taking $|t_j\rangle = |\psi\rangle$ and $|N_1\rangle = |M_0\rangle$, then from (5) we get $p(\psi, M_0) = 1$, whereas from (5′) we get $p(\psi, M_0) = |c_0|^4$.) It also implies that all observables N are compatible with M, *i.e.* all N commute with M.

econcile the Copenhagenist claim that the incoherent superposition of tates does *not* describe the state of the individual system and measuring nstrument—*i.e.* the claim that the entire system is now *really* in only one these states, whether we know it or not—with his claim that the original ure state vector from which this incoherent one evolved *did* describe the ate of the entire system. I have referred to this problem as the conceptual spect of the measurement problem.

The physical aspect of the problem has been solved by Daneri *et al.* 1962]) using an ergodic approach, and by Hepp ([1972]) using an gebraic-thermodynamic one. In the algebraic approach, for instance, ne considers the (C*) algebra of observables defined by the interacting ystem and measuring device and uses the given state of the system to onstruct (*via* the G–N–S construction) a Hilbert space. The Hamiltonian hen defines the time-ordered one-parameter group of automorphisms on he algebra of observables. In the thermodynamic limit (*i.e.* the number of articles in the measuring device $N \to \infty$, the volume of the device $V \to \infty$, with N/V constant) and taking the limit time $t \to \infty$, the original ure state converges (weakly) to an incoherent superposition of pure states.

There remains, however, the residual conceptual problem referred to bove. The Copenhagenist algorithm for applying quantum theory to physical situations, for *doing* quantum theory, stipulates that the state vector is to be attributed to an individual system so long as it is a pure tate, but that a 'mixed' state into which a pure state may, in a sense, volve, refers to an ensemble. The Copenhagenist thus, so to speak, vitches interpretations midstream. He begins with a state vector which e takes to be the state description of an individual s[illegible] [illegible]ws it to develop as a result of an interaction with a meas[illegible] [illegible]e, and, at a particular stage of its evolution (when it [illegible] [illegible]erent), he insists that the state vector is no longer to be regarded as describing the state of the individual system, but rather that of an ensemble of systems. He, the Copenhagenist, applies his rule consistently and logically (classical logic, of course). There is no ambiguity or vagueness about this procedure. But this switching of interpretations is intellectually unsatisfactory nevertheless.

I maintain that this switching of interpretations is the Copenhagenist's way of dealing with two conceptually different types of behaviour, or of

---

For microscopic systems M, (5) and (5′) are indeed incompatible—for there are observables of the system M (if it is microscopic) which are incompatible with the observable M, for the time reversal of (3) is in principle possible, and so on. The problem of measurement—or at least the physical problem as we have defined it—is, why are (5) nd (5′) compatible for macroscopic measuring devices? This is the question to which neri *et al.* and Hepp address themselves.

time developments, and that the Popperian too, though he may deal with the problem differently, is left with exactly the same intellectually unsatisfactory situation of two conceptually different types of behaviour.

Let us first consider the position of the Copenhagenist. For him the state vector embodies the past behaviour of the system (the equivalent set of preparation procedures) as characterised by that future behaviour which may be legitimately inferred from it. This past behaviour is such that some legitimate inferences can indeed be made for the individual system. Since the theory is individually indeterministic but statistically deterministic, there will be some future behaviour which cannot be predicted except in a statistical way. Finally, those inferences which can be made for the individual system suffice to determine, by virtue of the theory, the statistical behaviour. This is what the Copenhagenist means to say when he attributes the state vector to an individual system.

But the very future behaviour about which we speak is itself characterised by what still further future behaviour may be inferred from it. Thus, when we say that we cannot now, at time t_1, predict on the basis of its behaviour up to t_1 (its state at t_1) the future behaviour of the system at time t_2 when it is subjected to a particular measuring device, we are saying that given only the state now at time t_1, we can make *no* (relevant) legitimate inference about the individual system's behaviour after t_2. (For, if we could, then such an inference would characterise the system's behaviour at time t_2 and we could now predict on the basis of the electron's state at time t_1, its future behaviour at t_2; but, by hypothesis, this is impossible.) When we test at time t_2 for one branch of a pure state vector at time t_1 then, by definition, the state vector which embodies the past behaviour up to time t_1, becomes one for which *no* legitimate inferences may be made for the individual system; interference is no longer possible.

But this by itself is not enough. The system *does* behave in a particular way at time t_2. That is, from its behaviour at that time we can make legitimate inferences about behaviour subsequent to that time, inferences which could not have been made on the basis of behaviour prior to t_2. The state vector changes. What may be inferred for the individual system from its behaviour at time t_2 is different from what may be inferred from its behaviour at time t_1. This change in the state vector has to do with the exhibition of behaviour. It is due to the behaviour the system exhibits when it does or does not pass a particular filter. (Whether we have noted its passage through the filter is quite irrelevant; what is relevant is whether it has been subjected to a filter, *i.e.* whether a test has been made.)

But there is another, conceptually different, kind of change of state which concerns, not the exhibition of behaviour, but the relevancy of the past behaviour exhibited. We do indeed observe behaviour and can make certain legitimate inferences from such behaviour. But time goes on; the past recedes into the more remote past, and the set of inferences which may legitimately be inferred from that past behaviour changes. A particle passes a z-axis spin-up filter and is subjected to a device which rotates spin by 90° around the y-axis so that it is now x-axis spin-right. Insofar as we are interested in legitimate inferences concerning future spin behaviour, the past behaviour exhibited by this particle is equivalent to that exhibited by one which has passed a spin-right filter (and not been subjected to the rotating device). In this sense the above particle has exhibited one form of spin-right behaviour. But when we speak of the time development of the spin-up state into a spin-right one, we are differentiating between the exhibition of spin-right behaviour, as in passing the appropriate filter, and exhibition of spin-up behaviour whose relevancy to predictions changes (and in a deterministic way) by virtue of passage through the rotating device.

Similarly, the legitimate inferences concerning the spin behaviour of an x-axis spin-right particle do not change as the particle passes through a Stern–Gerlach apparatus oriented in the z direction, and this does not constitute exhibition of spin behaviour, not because we do not know it, but because it actually is not! The fact that the same inference may legitimately be drawn depends crucially on the fact that the particle does not exhibit spin behaviour as in the experiment schematised in Figure 1, where the electron will definitely pass through the filter on the right if no further spin behaviour is exhibited (other than its passage through the filter on the left) but might not do so if further spin behaviour *is* exhibited, such as would constitute a test for spin-up or down (whether or not we actually observe the results of the test).

There are thus two conceptually different types of changes of state. One which occurs whenever the system exhibits behaviour of the unpredictable sort, and in terms of which the Copenhagenist states are defined; the second which refers to the changing relevancy of the actual past behaviour exhibited. The first, by definition, is indeterministic and can occur only when the system interacts with a macroscopic device. The second is, by definition, deterministic, for there is a fixed rule, determined by the sort of interaction which the system undergoes (as defined by the Hamiltonian), which stipulates what we may legitimately infer as a function of the elapsed time between the time of the past behaviour exhibited and the time of the legitimately inferred future behaviour.

We try to bridge the gap between these two types of changes because we feel that both behaviour exhibited and the change in its relevance to future behaviour are due to the interaction of a system with its environment. We tend to feel that if behaviour can only be exhibited by interaction with a macroscopic device, then the peculiarities of this behaviour should be deducible from a suitable 'macroscopic limit' of macroscopic inter actions.

In one sense, the gap can be bridged. That is, we can and should insist that changes in the relevancy of past behaviour upon interaction with a macroscopic measuring device should be describable in terms of a suitable limit of the evolution of the relevancy of past behaviour determined by microscopic interactions. And this has been done. The work of Daneri *et al.* and that of Hepp have shown how when a system interacts with a macroscopic device for which exhibited behaviour is unpredictable, the relevancy of the past behaviour of the individual system for its future behaviour 'evolves' into 'irrelevancy' for that individual system; its relevancy is for statistical predictions.

In another sense, the gap cannot be bridged, for it is inherent in an indeterministic theory of the quantum mechanical sort. In such a theory there must of necessity be, in the exhibited behaviour of an individual system, a discontinuous break with the past. Since we can make inferences for the individual system on the basis of such behaviour, we can do no other than wipe the slate clean of descriptions of past behaviour and start afresh with the present observed behaviour.

All this is implicit in the Copenhagenist's treatment of the measurement process and state-vector reduction. He begins at t_1 with a state vector description which characterises the system's past behaviour by means of the predictions which can be made on the basis of that behaviour. Due to interactions, the predictions which can be so made change, and in the measurement process they change in such a way that no predictions at all can be made for the individual system on the basis of behaviour prior to or at t_1. This signals the fact that the system has exhibited new behaviour. What this new behaviour is at t_2 cannot, of course, be predicted on the basis of behaviour prior to t_2. To find out what it is, we must look at the measuring instrument. Thus, in one sense, this incoherent state vector still does apply to the individual system; it tells us what predictions can be made now at time t_2 on the basis of the system's behaviour at t_1, namely, nothing for the individual system but only statistical predictions for ensembles of systems. In another sense, this incoherent state vector is no longer the system's state; it no longer characterises the system's past behaviour, for the system has exhibited new behaviour at t_2.

This is the Copenhagenist's description of the situation. Does the Popperian avoid this dilemma? Not really, though he handles it differently. First, it is obvious that he, like the Copenhagenist, must supply (or believe in the existence of) an account of how a coherent superposition of states evolves in some macroscopic limit into an incoherent one. But more important, he too must accept as contingent fact that there are two different conceptual types of changes in time when dealing with individual systems.

For the Popperian the *psi*-function represents the statistical distribution of (Popperian) states within an ensemble. A particular distribution is achieved by a preparation procedure such as selection by a particular type filter. From the point of view of the individual electron it is to be characterised as a member of a particular ensemble with its particular statistical distribution of states on the basis of its past behaviour with respect to the preparation procedure. In this sense for the Popperian as for the Copenhagenist, the *psi*-function embodies the past behaviour of the individual electron as regards a particular preparation procedure. When an electron passes another filter, it has again exhibited behaviour as regards a preparation procedure. We therefore characterise the electron as a member of a different ensemble, one prepared in a different way, and for this reason the ψ-function changes.

The ψ-function changes in yet another way. When a spin-up electron passes through a device which rotates spin by 90° around the y-axis the ψ-function changes in a deterministic way. Here too the ψ-function changes as a result of exhibited behaviour in the electron. The spin-up ψ-function is to be associated with a preparation procedure such as that of selection by a spin-up filter, not by that of selection by spin-up filter and passage through the forementioned device. This latter preparation procedure is in fact equivalent to the x-axis spin-right preparation procedure. Yet this change is deterministic and the former type change is not.

The Popperian would explain the difference in this way. In the indeterministic case, the second filter poses a question to the electron: will it or will it not pass the filter? By answering this question, by behaving in a particular manner, the electron has supplied us with more information about itself. We now know in which subensemble of the previous ensemble the electron was. In the deterministic case no question has been posed to the electron. Of course, the statistical distribution of Popperian states in an ensemble of spin-up electron changes as it passes through the spin-rotating device. But because each Popperian state changes in exactly the same way, because in fact the situation is deterministic, we have no further information about our particular electron relative to the ensemble: *no question has been posed to it.*

We thus have two types of systems with which the electron can interact; those which do not pose questions to electrons and those which do.

And Popperian states are defined with respect to these latter type systems; they are truth valuations over the set of questions which may be posed to the electron. The Popperian measurement problem may be phrased as follows: since a system which poses questions must be macroscopic (though not all macroscopic systems with which the electron may interact relevantly need pose relevant questions) and thus any microscopic system is necessarily not of the sort which poses questions, what is there about a macroscopic measuring device, described in terms of its microscopic constituents, which turns it into a device of the sort which can pose questions?

We may pose this question to Popperian$_{\text{I}}$ in the following form. Interaction with a microscopic device is necessarily deterministic and the individual Popperian states evolve in a well-defined manner. If, for instance an electron which has emerged from a spin-up filter is in a state of both spin-up and spin-right and is subjected to the spin-rotating device, its state will change to one which is both spin-right and spin-down. This is in fact, for the Popperian, one way of testing whether it was spin-right by subjecting the electron to a device which rotates spin by 90° around the *y*-axis and testing for spin-down. Yet a macroscopic device changes the individual Popperian state in an indeterministic manner. Insertion of the Geiger-counter in the upper-channel in the experiment schematised in Figure 1 brings about, according to Popperian$_{\text{I}}$, an interaction which for at least some electrons alters the state from spin-right to spin-left. How do we get an indeterministic altering of states from a macroscopic limit of microscopic deterministic interactions?

We cannot! Popperian$_{\text{I}}$ cannot deal with this problem because he is treating the two types of interactions, that with a microscopic system and that with a macroscopic measuring device, in a conceptually similar manner. They are both devices which alter the individual states of the electron. And one cannot then *do* quantum mechanics by merely considering a macroscopic measuring device as some suitable limit of microscopic constituents. Popperian$_{\text{II}}$ solves the problem, as does the Copenhagenist, by treating these two types of interactions in a conceptually different manner. Interaction with a microscopic system alters the individual Popperian states and in a deterministic manner. Interaction with a measuring device alters not the state, but the question with respect to which the states are defined. (As in the experiment of Figure 1, where as mentioned above, the question asked of the electron when no Geiger-counter is inserted in the upper channel is: will the electron pass through the lower channel and afterwards the filter at the extreme right? Whereas, if a Geiger-counter is inserted in the upper channel the question is:

will it pass through the lower channel and pass the filter at the right if a Geiger-counter is placed in the upper channel?)

Like the Copenhagenist, the Popperian must still solve the physical problem. Both of them must insist that the physical results obtained by treating (conceptually) the macroscopic device as a suitable limit of microscopic constituents must be consistent with ignoring the conceptual peculiarities of the macroscopic measuring device. For the Copenhagenist this means that if a macroscopic measuring device is not considered as a means for the electron to exhibit behaviour on the basis of which future behaviour is inferred (*i.e.* we do not look at the results obtained from the measurement), but merely as a system with which the electron interacts (and thus changes the relevancy of the past behaviour exhibited), then the past behaviour should 'evolve' into irrelevance for the individual electron and be relevant only to statistical predictions. For the Popperian this means that if we consider the measuring device, not as one which poses questions (*i.e.* we don't look at the result), but merely a system which alters individual Popperian states in a given prepared ensemble in a deterministic way (as in the experiment of Figure 1, where the insertion of the Geiger-counter may be treated as altering the individual Popperian states in an ensemble, so that half of them are now spin-right and half spin-left), then the statistical distribution of states for an ensemble obtained in this way should be the same as those given by the conditional probability rule mentioned at the end of section 4. Both these demands are the same. They demand an account of how in some suitable macroscopic limit, one derives the evolution of a pure state into an incoherent superposition of pure states. And this demand has been satisfied.

But the Popperian has not bridged the conceptual gap any more than has the Copenhagenist. He cannot explain why, though microscopic interactions cannot pose questions, a macroscopic combination of these can. The Popperian can only state that a measuring device poses questions because that is what we mean by a measuring device, and such a *concept* is *not* derivable by taking some suitable limit of concepts referring to microscopic systems only.

SUMMARY

I first set out two diverse notions of state and constructed on the basis of these notions two conceptual schemes for describing quantum theory. The Copenhagenist and Popperian as I have described them stand, so to speak, back to back, each viewing quantum theory from his own perspective. The Copenhagenist state characterises the past behaviour of the system. It is true that that past behaviour is itself characterised by the legitimate predictions which can be made on the basis of it. Nevertheless, the state description is a description of past behaviour. If we know the Copenhagenist state, we know the system's relevant past behaviour (but not all we might wish to know about relevant future behaviour). The Popperian state characterises the future behaviour of the system. If we know the Popperian state, we know how the system would behave in *all* relevant situations. And though, by testing for behaviour, we acquire information about past behaviour, the Popperian state in itself at a particular time still tells us nothing about the system's past behaviour.

Because of their contrasting perspectives, the Copenhagenist and Popperian are at odds in their description of quantum theory: observables and the question of whether they always have a value in the system or not; the problem of what sort of information measurements convey about the state of the system; the problem of whether the quantum state vector describes a state of an individual system or of an ensemble; and the problem concerning the objective probabilities of quantum theory, whether they are properties of individual systems or of preparation procedures and whether they change or not upon measurement are all treated quite differently by the Copenhagenist and Popperian. Yet each, in his own manner, is quite consistent; each can describe quantum theory consistently within his own conceptual scheme and both can agree about all physical predictions for the theory. The physics—at least for the schemes we have constructed—is the same.

I then treated the projection postulate and the measurement problem. Observation, I have maintained, does play a unique and fundamental role in quantum theory, not in the subjective sense of noting the result of a measurement, but rather in the objective sense of subjecting the system to a measuring device. Mathematically, this is expressed by the isomorphism between questions and preparation procedures; and physically, by the fact that answering a question *is* a preparation procedure and vice-versa.

The Copenhagenist incorporates this 'peculiarity' into his very language. A system is in a Copenhagenist state if and only if it has been subjected to an appropriate measuring device and exhibited the appropriate behaviour. Thus in the experiment of Figure 1, if no Geiger-counter or any other measuring device is placed in one of the channels, *the electron does not pass through either of the channels.* This is not meant in the sense that we do not know through which channel the electron has passed, but in the sense that the electron has not been subjected to a measuring device which tests through which channel the electron has gone. To say that the electron has either passed through the upper channel or through the lower one is, in Copenhagenist language, to say that the electron has been subjected to a measuring device which tests through which channel the electron has passed. This is not true for our electron and therefore (speaking as a Copenhagenist) it has neither gone through the upper channel, nor has it gone through the lower one. What it has done is to have passed a z-axis Stern–Gerlach apparatus and been subjected to a device with two channels as schematised in Figure 1. ***This then is the Copenhagenist state, i.e.*** its past behaviour. (Mathematically, this is expressed as the usual coherent superposition of upper and lower channel states.)

If Geiger-counters were, however, placed in the channels, the electron would show up in one and only one of them. Thus for the Popperian, the electron does go through one of the channels whether or not a test of this is made (though, of course, we do not know which one unless the test is in fact made). The Popperian's description of the 'peculiar' role of observation in quantum theory is somewhat different from that of the Copenhagenist. When considering the behaviour of going through the channel as just one aspect of a total overall behaviour which also involves other spin-behaviour, the Popperian will have to differentiate between passage through a channel without a test of this being made and passage through the channel where a test is made. The projection postulate is indeed unique to quantum theory. We may grant to Popper that even in classical probability theory $p(B/A)$ is in general different from $p(B)$. But our Popperian would have to add that in quantum theory there is the further difference that $p(B/A')$ is in general also different from $p(B/A)$, where A' refers to behaviour A as tested and 'tested' means 'subjected to a measuring device'.

I have asserted here, as elsewhere (see my [1972]), that the measurement problem has two aspects to it. The first, which we have referred to as the physical problem, has to do with the development of a pure state into an incoherent one. For the Copenhagenist, as for the Popperian, whether we take the state vector as an individual system's state or as an ensemble's, the problem is the same; a pure state vector (whether for a single combined system of measured and measuring systems, or for an ensemble of such combined systems) evolves, in the measuring process, into an incoherent state vector. This, seemingly, is incompatible with the Schrödinger-type time-development for the microscopic constituents of the macroscopic measuring device. I have cited two solutions of this problem offered in the literature (Daneri *et al.* [1972] and Hepp [1972]). They both seek to resolve *this* problem by showing how a pure state can be considered as evolving into an 'impure' one by taking a suitable 'macroscopic limit' of interactions involving microscopic constituents.

The residual conceptual problem, I have asserted, is inherent to quantum theory, precisely because measuring devices play a special role in this theory and thus cannot be treated as merely some 'macroscopic combination' of microscopic constituents. The problem for the Copenhagenist is to explain why he switches interpretations of the state vector; how a state vector which he, the Copenhagenist, attributes to an individual system, develops into one which he attributes to an ensemble. The Copenhagenist answer is that when a pure state develops into an 'impure' one, this signals the fact that the system has been subjected to a measuring device and thus has exhibited *new* behaviour and must thus be described by a new state vector—the one shown by the measuring device. The Popperian is faced with a similar problem of explaining why A as tested must be treated, in quantum theory, differently from A as untested, where, as cited above, 'tested' means 'subjected to a measuring device'. For both the Copenhagenist and Popperian, the unique role of measuring devices in quantum theory leaves a conceptual gap between 'measuring device' and 'microscopic constituents'—on the one hand, the gap between measuring devices which elicit Copenhagenist behaviour and pose Popperian questions, and on the other hand their microscopic constituents which do neither.

Ben-Gurion University of the Negev

REFERENCES

ABBOT, J. C. [1969]: *Sets, Lattices, and Boolean Algebras*. Boston: Allyn and Bacon.

AHARONOV, Y., PENDELTON, H. and PETERSEN, A. [1969]: 'Modular Variables in Quantum Theory', *International Journal of Theoretical Physics'* **2**, p. 213.

AHARONOV, Y. PENDELTON, H. and PETERSEN, A. [1970]: 'Deterministic Quantum Interference Experiments', *International Journal of Theoretical Physics*, **3**, p. 443.

BELL, J. S. [1966]: 'On the Problem of Hidden Variables in Quantum Mechanics', *Review of Modern Physics*, **38**, p. 447.

BELTRAMETTI, E. G. and CASSINELLI, G. [1972]: 'On the Logic of Quantum Mechanics', *Istitute di Scienze Fisiche dell Universitadi preprint*, Geneva.

DANERI, A., LEINGER, A., PROSPERI, G. M. [1962]: 'Quantum Theory of Measurement and Ergodicity Conditions', *Nuclear Physics*, **33**, p. 297.

FEYNMAN, R. P. [1965]: *The Feynman Lectures on Physics, Vol. III*. Reading, Mass.: Addison-Wesley.

FREUNDLICH, Y. [1972]: 'Mind, Matter, and Physicists', *Foundations of Physics*, **2**, p. 129.

FREUNDLICH, Y. [1977]: 'Two Views of Objective Quantum Theory', *Foundations of Physics*, **7**.

GARDNER, M. R. [1972]: 'Quantum-Theoretical Realism: Popper and Einstein *v.* Kochen and Specker', *The British Journal for the Philosophy of Science*, **23**, p. 13.

GLEASON, A. M. [1957]: 'Measures of Closed Subspaces of Hilbert Space', *Journal of Mathematics and Mechanics*, **6**, p. 885.

HEPP, K. [1972]: 'Quantum Theory of Measurements and Macroscopic Observables', *Helvetia Physica Acta*, **45**, p. 237.

JAUCH, J. M. [1968]: 'States and Observables', chapter 3 of *Foundations of Quantum Mechanics*, Reading, Mass.: Addison-Wesley.

JAUCH, J. M. and PIRON, C. [1970]: 'What is "Quantum-Logic"?' in P. G. O. Freund, C. J. Goebel, Y. Nambu (*eds.*): *Quanta*. Chicago and London: University of Chicago Press.

KOCHEN, S. and SPECKER, E. P. [1967]: 'On the Problem of Hidden Variables in Quantum Mechanics', *Journal of Mathematics and Mechanics*, **17**, p. 59.

MACK, D. [1971]: 'Natural States and Past-Determinism of General Time Systems', *Information Sciences*, **3**, p. 1.

PARK, J. L. [1968]: 'Nature of Quantum States', *American Journal of Physics*, **36**, p. 211.

PARK, J. L. and MARGENAU, H. [1968]: 'Simultaneous Measurability in Quantum Theory', *International Journal of Theoretical Physics*, **1**, p. 211.

POPPER, K. R. [1967]: 'Quantum Mechanics Without the "Observer" ' in M. Bunge (*ed.*): *Quantum Theory and Reality*. Berlin and Heidelberg: Springer-Verlag.

VON NEUMANN, J. [1955]: *Mathematical Foundations of Quantum Mechanics*. Princeton, New Jersey: Princeton University Press.

WIGNER, E. P. [1967]: 'Remarks on the Mind-Body Question' in Joe Bloggs (*ed.*): *Symmetries and Reflections*. Bloomington, Indiana: Indiana University Press.

Brit. J. Phil. Sci. 29 (1978), 179–184 *Printed in Great Britain*

Discussions

ACCURACY OF PREDICTION: A NOTE ON DAVID MILLER'S PROBLEM

Suppose we have two false quantitative theories A and B both of which predict (incorrect) values for some common set of physical magnitudes. It is generally assumed that the extent of the departure of these predicted values from the *actual* (or observed) values of the magnitudes in question can provide a means of assessing the relative merits of A and B. But the question is—how? What possible features of the relations among the set of predictions of A, the set of predictions of B and the set of true values could allow us to decide which of A or B is the better theory? Important as this question undoubtedly is for science it seems that a satisfactory general answer has yet to be given, even though in many specific cases the question can, apparently, be answered readily enough.

In a recent paper ([1975]), David Miller has paid detailed attention to a putative, and apparently simple, special case. There will be instances, we might have supposed, where the following relation holds between the two false theories. The predictions of one of them—B say—are never further from the true values than are those of A, and are sometimes closer. That is, *theory B is uniformly more accurate than theory A*. In such a case, Miller points out, it is eminently plausible to suggest that B is the better theory, that it approaches more closely to the truth. He then proceeds to demolish this suggestion. Now there are arguments to show that even if theories A and B were related in the way just described this fact could not of itself compel our acceptance of B as the better theory. However Miller does not discuss such arguments, for he has a more striking demonstration that this suggestion of using uniformly greater accuracy as a criterion for choosing between rival false theories is quite useless. He shows that if A and B differ from each other in a sufficiently interesting way, they *cannot* stand in such a relation to one another. Neither theory can be uniformly more accurate than the other; *the putative special case does not exist.*

At the core of Miller's argument is his demonstration of a simple mathematical device. He shows how, given two independent physical magnitudes (or parameters), Θ and Φ say, on which theory B is the more accurate, we may always *define*—in terms of Θ and Φ—two new magnitudes Ψ and X, for which the rival theory A will give the more accurate predictions. It is the purpose of this note to make some remarks about Miller's result. In particular I wish to draw attention to a feature of his definition (of Ψ and X) which may not have been generally noticed. Recognition of this feature allows a somewhat different interpretation from that given by Miller of what his method of construction achieves.

To begin with, it will be useful to observe that there are, on the face of it, *two* distinct ways whereby it can arise that neither of two contending false theories is uniformly more accurate than the other. I shall describe each in turn.

In case I, theories A and B predict values for some common *parameter* throughout the whole range of an independent variable, and while A is the more accurate for some *parts* of the range, B is more accurate for the others.

That is, neither theory has a monopoly of accuracy for the one parameter. A particular example is illustrated in figure 1, in which curves representing the predictions of *A* and *B* are shown in relation to the true values of a *t*-dependent parameter $\Theta(t)$. This is not, of course, an example of the sort which Miller sets out to discuss in his paper. It will however be useful to make some comments about it before going on to describe case II.

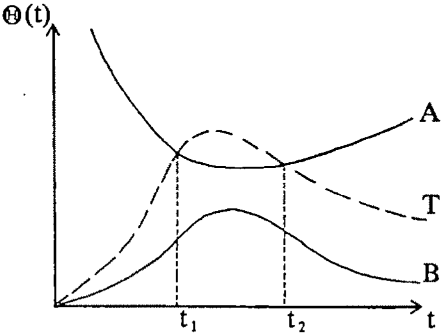


Figure 1.

At least for examples as clear as that of figure 1 there seems to be no problem in deciding which is the better theory. The fact that *A* is undoubtedly more accurate than *B* in the range t_1 to t_2 is unlikely to give us cause to hesitate in rejecting it in favour of *B*. If challenged on the choice we should no doubt point to the several features that curves *B* and *T* have in common—a single peak, zero value at the origin, *etc.*—which are not shared by *A*. This is *not* to say that such considerations settle the matter. In the first place it will not *generally* be true that the pattern of similarities among the curves will allow such an apparently clear cut decision. But further, even in examples life that in figure 1, it is far from clear why those features which were picked out should be counted as decisive. Indeed, in the absence of a demonstration to the contrary, it is possible to be sceptical as to whether any justification for it could be found at all. The shapes of the curves are dependent upon the choice of metrics for assigning values to *t* and $\Theta(t)$. Might not there be some drastic transformation (equivalent to the adoption of alternative metrics) that so changes the shapes of curves *A*, *B* and *T* as to render *A* more closely similar to *T* than *B* is?[1] Now the practice of deciding between rival theories in just the way described is very common in physical science, and thus the question of its justification demands attention. This need will be further highlighted by conclusions later in the paper. Nevertheless, it is not this question which concerns me here. The obvious lesson I wish to take from considering the example of figure 1, and this is the important point from the viewpoint of Miller's problem, is simply this. In the practice of choosing between rival theories in type I cases it is not *uniformity* of accuracy, or the lack of it, which is taken to be significant, but rather the pattern of relations among the three curves *over the whole range of the independent variable.*

---

[1] I owe this point to David Miller. I am indebted to him for many useful discussions of this topic, and especially for his painstaking and helpful criticism of various drafts of this note.

We now come to case II. This is the state of affairs which Miller describes. Here the failure of either theory to have a monopoly of accuracy is due to the fact whilst there are some physical magnitudes (either constants, or parameters throughout the entire range of the associated independent variables) on which theory A is more accurate, there is also a ***totally distinct set of magnitudes*** for which theory B enjoys the success.

Now this latter case strikes one initially as less familiar and more puzzling than the first. How should we adjudicate between theories one of which is better at predicting, say, the specific heats of metals and the other at predicting coefficients of thermal expansion? But the important and startling result produced by Miller is that if the rival theories do not give rise to the situation of case I, then (provided only that they differ in a sufficiently interesting way) we *inevitably* face the situation of case II. He has shown, for example, that should any discrepancy (not attributable to experimental error) between the predictions of special relativity theory and observation come to light, we shall at once be in a position to specify a set of magnitudes for which classical mechanics will give *better* predictions than will relativistic mechanics. That alone might well surprise us. But there appears to be an accompanying threat with deeply sceptical implications. The suggestion is that the discovery of this new set of magnitudes will remove any ground for regarding classical mechanics as inferior to relativistic mechanics. Neither theory will then have a claim to being closer to the truth. That is to say that in case II, in contrast to case I, the fact that uniformly greater accuracy cannot be ascribed to one of a pair of rival theories does appear to matter. Since, as Miller argues, there are no grounds for regarding Ψ and X as any less respectable or significant parameters than are Θ and Φ, then neither, apparently, can there be grounds for counting A's greater success in predicting Ψ and X as any less impressive than B's with respect to Θ and Φ (nor, of course, *vice versa*). Thus the problem is that since the order of success of two theories can switch as different parameters are selected for measurement there can apparently be no hope of grading the theories in terms of closeness to the truth in any way which is not relative to a particular set of parameters.

What I intend to show below is that this particular difficulty disappears when Miller's method of construction of Ψ and X is re-examined. I shall claim that it can be re-interpreted in a way that allows the distinction between cases I and II to be erased. Case II can be assimilated to case I. In view of the fact that the problem of giving an account of the appropriate criteria for a choice of theories in examples of case I may well be formidable, the gain from this assimilation is at best a modest one. Indeed, should the problem of case I prove intractable, then the move is futile. I shall be content however to show that Miller's demonstration, intriguing though it undoubtedly is, poses no *new* problem.

The point can be made by reference to the expressions (28) and (29) in Miller [1975]. These give examples of new magnitudes Ψ and X defined in terms of Θ and Φ. (28), for instance, reads

$$\Psi(t) = \Theta(t)+\mu \cdot \frac{\vartheta_C(t)-\vartheta_A(t)}{\phi_A(t)-\phi_C(t)} \cdot \Phi(t)+(1-\mu) \cdot \frac{\vartheta_A(t)-\vartheta(t)}{\phi(t)-\phi_A(t)} \cdot \Phi(t). \qquad (28)$$

Similarly, (29) defines $X(t)$, replacing μ by ν on the right hand side. In these expressions ϑ, ϑ_A, ϑ_C, ϕ, ϕ_A and ϕ_C are supposed to be functions of the variable

t only. That is, in Miller's terminology, they are all t-determinate designators. Now Miller's original intention, was that μ (in (28)) and ν (in (29)) should be assigned ***distinct fixed*** numerical values each within the range 0 to 1. This then gives the desired result. The definitions generate two distinct mutually independent parameters Ψ and X to set alongside the other pair Θ and Φ against which to test the two false theories (denoted by A and C at this point of Miller's account) for order of approximation to the truth, ***and the two pairs lead to opposed answers.***

However, an alternative view of expressions (28) and (29) is as follows. There is no obvious reason not to allow μ (or ν) to ***vary continuously*** over an indefinite range of values. If this happens we then have effectively a single physical parameter, but one whose value depends not only upon t, but on a further variable μ. We can indicate the algebraic form of (28) (which on this view is of course only notationally different from (29)) as

$$\Psi = \Theta + f(t, \mu) \,.\, \Phi \tag{28'}$$

where f is a function of the two variables t and μ. It is instructive to consider the properties of Ψ as seen from this viewpoint in their bearing on the problem of verisimilitude.

First of all, there can be no objection to Ψ's being dependent upon two variables. Almost always Θ and Φ will themselves be dependent upon more than one; it is simply that we commonly pay attention to one at a time. Nor should there be any worry about μ's being dimensionless. This does not debar it from being itself a physical magnitude no less than t is. The use of variable dimensionless quantities for physical parameters is commonplace in science—'reduced' temperature in thermodynamics, or Mach number in aerodynamics, for example. Thus Ψ in its new guise remains perfectly acceptable as a physical magnitude.

But the chief point of significance for the problem of accuracy of prediction is this. In considering the relative degrees of success with which the two false theories predict values for the new parameter $\Psi(t, \mu)$ we now seem bound to examine what happens as μ varies throughout its entire range, rather than at two isolated values of μ in the restricted range 0 to 1 as Miller's discussion invites. It is a matter of simple algebra to determine those general features of the pattern which are of interest to us here. We find, in fact, that although within the interval $0 < \mu < 1$, as Miller shows, the order of accuracy of the two theories in predicting $\Psi(t, \mu)$ is the reverse of that which obtains for prediction of Θ and Φ, outside this interval the original order is restored for at least part of the remaining ranges of μ. Thus we have now reached a situation where the relative fortunes of two false theories in predicting the value of a single parameter switch as we move over the whole range of an independent variable. That is, it is now ***no different from case I.***

Thus we may conclude as follows. The consequences of Miller's result have been shown to be less novel than might be thought, and *a fortiori* to provide no new grounds for pessimism about the possibility of comparing false theories. On the other hand he has shown with great force that to expect of any false theory that it be uniformly more accurate than any other is always a serious mistake. There can never be such a simple answer to the problem of comparability of theories. A consideration of case I is thus shown to be the more urgent. And finally I think that his method strongly suggests a somewhat novel view of physical magnitudes. The situation we are now faced with can perhaps be described in the following terms. The language in which scientific theories are couched leads us naturally to think of different types of physical quantity as absolutely distinct. The kinetic energy of a body is one sort, its volume is quite another. And yet if constructions of the kind given by (28′) are acceptable then it follows that any two magnitudes whatever are members of indefinitely many continuous chains of parameters which link them. It is as if all possible magnitudes are points in an infinite-dimensional continuous manifold. An experimenter, in testing theories, chooses to make observations at various selected points in this manifold. According to what criteria are these points selected? What are the desiderata? Should, for example, the points be scattered as widely as possible throughout those regions of 'magnitude space' to which the theories relate? I suggest that questions of this kind require urgent attention.

DERYCK HORTON
Lanchester Polytechnic
University of Warwick

REFERENCE

MILLER, D. [1975]: 'The Accuracy of Predictions', *Synthese*, 30, pp. 159–91.

SAMPSON'S 'DILEMMA'

In a recent issue of this *Journal*, Sampson ([1975]) poses a problem which, he claims, arises for a number of methodologies which he surveys. This problem is that of theory-choice in a two-level science, and can be motivated as follows: Linguists construct theories at two different levels. At the first level, linguists construct theories (grammars) of individual languages. A theory t of some language L defines a set S of strings of words over the vocabulary of L and predicts (*ceteris paribus*) that each string s in S is in fact in L (*i.e.*, will be used by speakers of L, other things being equal). We test (*i.e.*, try to refute) t by trying to find strings in L which are not in S. Many different grammars can be constructed for any given language which will not be known to be refuted in this way. Thus, the linguist will appeal to various methodological criteria (*e.g.* strength, simplicity, *etc.*) in deciding which of the (so far) unrefuted grammars to prefer. At the second level, linguists construct theories about grammars. Such a theory T defines a set Σ of possible grammars and predicts (*ceteris paribus*) that every actual human language has an unrefuted grammar in Σ. We test (*i.e.*, try to refute) T by trying to find languages which do not have any unrefuted grammars in Σ. Different second-level theories of this kind can be constructed which will not be known to be refuted in this way. Thus, the linguist will appeal to various methodological criteria in deciding which of these (so far) unrefuted theories to prefer.

Sampson's Problem can now be formulated as follows: Suppose we discover some language L, such that

(*i*) There are, for instance, unrefuted grammars t and t' of L.
(*ii*) t is in the set Σ defined by the unrefuted theory T, but not in the set Σ defined by the unrefuted theory T'.

(*iii*) t' is in the set Σ' defined by T', but not in the set Σ defined by T.
(*iv*) t is (methodologically) preferable to t'.
(*v*) T' is (methodologically) preferable to T.

According to Sampson, we now have the following dilemma: (1) T, T', t, and t' are all unrefuted. (2) Our methodological criteria bid us to prefer t to t' and also to prefer T' to T. (3) But these two (methodologically motivated) preferences are inconsistent, for t is not in the set Σ' defined by T'. (4) Thus, our only consistent choice options appear to be *either* to prefer the $\langle t, T\rangle$ pair to the $\langle t', T'\rangle$ pair, *or* to prefer the $\langle t', T'\rangle$ pair to the $\langle t, T\rangle$ pair. (5) But both of these choices are suboptimal (since, the first choice entails preferring a methodologically inferior second-level theory, while the second choice entails preferring a methodologically inferior grammar). (6) Furthermore, choosing between these two options is underdetermined methodologically (since, because neither option is dominant with respect to the other, there is no unique methodologically best choice). (7) Thus, methodology seems to fail us here. Thus, the 'dilemma' that Sampson's problem poses—*viz.* that there are problems of theory-choice that a range of methodologies fail to provide a solution for.

I claim that we can dissolve this 'dilemma' in terms of the following observations: (A) Sound methodologies *rightly* condemn the $\langle t, T'\rangle$ choice as inconsistent. (B) Sound methodologies *rightly* brand the $\langle t, T\rangle$ and $\langle t', T'\rangle$ choices as suboptimal. (C) Sound methodologies are *rightly* indifferent between the $\langle t, T\rangle$ and $\langle t', T'\rangle$ choices. (D) But Sampson's problem poses no *dilemma* for any sound methodology, which, in view of (A) and (B), will direct the linguist to reject *all three* of the choices which Sampson seems to think exhaust those available to the linguist. Instead, it will direct the linguist to try to construct theories t^* and T^* such that:

(*a*) t^* is an unrefuted theory of L.
(*b*) T^* is an unrefuted second-level theory.
(*c*) t^* is in the set Σ^* defined by T^*.
(*d*) t^* is (methodologically) as good a theory of L as t is.
(*e*) T^* is (methodologically) as good a theory as T'.

In this case, the pair $\langle t^*, T^*\rangle$ is methodologically preferable to the suboptimum pairs $\langle t, T\rangle$ and $\langle t', T'\rangle$ and to the inconsistent pair $\langle t, T'\rangle$. Thus, there is a unique best choice among these *four* options, and this choice is both optimum and consistent.

In short, the problem Sampson has described poses a dilemma for the methodologist only given an (unwarranted) assumption about the kinds of methodological advice a sound methodology can in fact give. When we abandon this assumption, we find that a sound methodology can provide an unequivocal, methodologically optimal solution to Sampson's problem.

F. B. D'AGOSTINO
London School of Economics

REFERENCE

SAMPSON, G. [1975]: 'Theory choice in a two-level science', *British Journal for the Philosophy of Science*, **26**, pp. 303–18.

Review Article

THE WHEEL AND BEYOND*

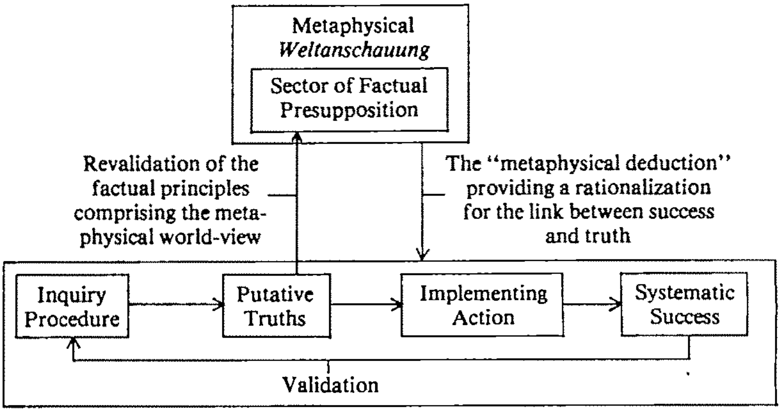
This ambitious book tackles a central question of epistemology, the question of rational warrant: what entitles us to claim to have knowledge about the world? Rescher's solution, as his title economically indicates, is *methodological*, concerned with the evaluation of methods of inquiry rather than with the evaluation of knowledge–claims, and *pragmatic*, stressing that the justification of a method lies in its successful application.

In general, Rescher argues, a method is warranted if it works, and must be assessed by the worth of its products. In the special case of cognitive methods, however, the 'wheel argument' dashed any hopes one might have entertained of justifying methods of inquiry by showing that they yield truths; for how is one to show that their results *are* true, except by means of those very methods? This, Rescher urges, obliges one to recognise that, in the evaluation of methods of inquiry, priority must be given to success in the practical, rather than the purely cognitive, objectives of inquiry. This practical success must be general, over a whole range of applications, though, by way of compensation, an occasional failure can be condoned; for a method is essentially public and general in application, so that its success should be reckoned wholesale rather than retail. Even in evaluating the practical success of a method, of course, one will inevitably need to have recourse to facts about what was done and what happened; but this, Rescher thinks, does not re-introduce the threat of circularity: one needn't insist on 'validated truths', but can rely on provisionally accepted 'plausible presumptions' about what the upshot of the use of a method in fact is.

Rescher does not think that the truth of a thesis consists in its being warranted by a successful cognitive method; he accepts an orthodox, correspondence account of the meaning of truth. So he faces a real question about the connection between the pragmatic success of a cognitive method and the truth of its results. This question he answers by means of a 'metaphysical deduction': given certain metaphysical principles, principles, on the one hand, about the character of man as a cognitive agent, and, on the other, about the external world, the practical utility of a method can be shown to be indicative of the truth of its products. Briefly, Rescher's metaphysical picture is of a community of cognitive agents interacting with their environment, sensitive to the consequences of their intervention, and constant in purpose, and of a world which is responsive to human manipulation, uniform, and 'nonconspiratorial'. But, now, how are these metaphysical theses themselves to be justified, if not by way of the products of successful cognitive methods? Here, Rescher argues, one has, not a vicious circle, but a series of feedback loops of impeccable virtue:

---

* Review of N. Rescher [1977]: *Methodological Pragmatism.* Oxford: Blackwell. £10.50. Pp. 315.


Metaphysical *Weltanschauung*
Sector of Factual Presupposition
Revalidation of the factual principles comprising the metaphysical world-view
The "metaphysical deduction" providing a rationalization for the link between success and truth
Inquiry Procedure
Putative Truths
Implementing Action
Systematic Success
Validation

[fig 4, p. 10].

Apparently the metaphysical presumptions of inquiry are only partially—only insofar as they are factual—supported by the success of the methods of inquiry which they rationalise (for no arrow leads *to* the metaphysical component); and it is, indeed, difficult to think what could either confirm or disconfirm, for example, the thesis that nature is nonconspiratorial, 'indifferent to our cognitive efforts'.

This metaphysical picture also, Rescher urges, lends support to a kind of methodological evolutionism, for, if the community keeps its objectives constant and adjusts its methods in the light of their successes and failures, then the survival of a method will be an indication of its pragmatic superiority; Rescher's evolutionism is, however, rather Lamarckian than classically Darwinian, for the selection of methods is inherently purposive and rational.

Although, by way of his 'metaphysical deduction', Rescher argues that the utility of a method justifies confidence in its products, his epistemology is fallibilist. It is possible that claims to knowledge, even perfectly justified claims to knowledge, should turn out to be mistaken (though in some specific cases the possibility of error is purely theoretical, and one is entitled to practical certainty). But Rescher stops short of scepticism. While, as a fallibilist, he allows that claims to knowledge are defeasible, he urges that the sceptic, who, he thinks, takes a perverse view of the burden of proof, refusing to give evidence its proper prima facie weight, is 'profoundly irrational'. Rather surprisingly, Rescher confines his discussion to 'thesis scepticism', and does not consider methodological scepticism of the kind advocated by Feyerabend.

With its stress on *praxis*, its fallibilism and its evolutionism Rescher's epistemology obviously has strong affinities with traditional pragmatism. Rescher believes, however, that by shifting the emphasis to methods he avoids the worst difficulties of 'thesis pragmatism'. James's proposal to identify the truth of a thesis with its utility, he argues, fails because utility isn't well-behaved enough (*e.g.* the conjunction of separately useful beliefs mayn't be itself useful). And Peirce's proposal to locate the truth of a thesis in its capacity to survive the process of scientific inquiry fails because, in view of the enormous range of alternatives, survival can be only weakly truth-indicative at best. *Methodological* pragmatism avoids these difficulties by grounding the truth of theses indirectly, by means of the practical success of the methods of which they are the products. There is room for dispute about Rescher's interpretation of James, whose view of truth, I think, was closer to Peirce's than Rescher suggests; but another point of interpretation, which has broader ramifications, is worth pausing longer over. Rescher distinguishes between the definition of truth, which he takes to be correctly given by the correspondence theory, and criteria of truth; and he seems to take the pragmatists to have been offering, as definitions of truth, what are at best weak and indirect criteria of truth. But the pragmatist theory of meaning requires that meaning be identified with criteria: a point, however, which Rescher apparently fails fully to appreciate (for example, he claims that Dewey distinguished truth and warranted assertibility, whereas, in fact, he proposed to identify them). If the meaning of 'true' is identified with our criteria of truth, however, it becomes impossible for a thesis to pass our tests and yet be false, or to fail them and yet be true; fallibilism, that is, is compromised.

Rescher's fallibilist, evolutionary theory of scientific inquiry as approaching a truth conceived in terms of correspondence has marked affinities, also, with Popper's epistemology. (But Popper is aware, as Rescher apparently is not, of the threat to fallibilism posed by a criterial theory of meaning.) But Rescher argues that Popper, too, gets into difficulties by concentrating on theses rather than methods. Popper's 'thesis Darwinism', he argues, is quite incapable of explaining man's cognitive success: elimination of falsehood would promise progress towards the truth only if the possibilities were limited. *Methodological* evolutionism avoids this difficulty by concentrating upon selection from among (not all the possible, but) the limited number of actually available, really promising methods. One may suspect that the crucial shift here is not, as Rescher thinks, from theses to methods, but from the logically possible to the actually feasible; and this suspicion is confirmed when one recalls Peirce's attempt to impose a limit on the infinitely many possible explanatory hypotheses by appeal to man's 'guessing instinct', his capacity to distinguish the half-way plausible from the logically possible but hopeless hypothesis.

However, an approach which, like Rescher's (and Peirce's), gives an important place to the cognitive agent has, I think, a significant advantage over one which, like Popper's, deliberately neglects the knowing subject. Fallibilism, as I understand it, is the interesting thesis that our cognitive methods are imperfect, that we, as cognitive agents, are apt to hold false beliefs. But concentration on the propositions believed at the expense of the knowing subject leaves room only for the surely much less interesting thesis that some of our beliefs are possibly false. A fallibilist epistemology needs a knowing subject.

Surprisingly, perhaps, since his enterprise depends so heavily upon it, Rescher takes the distinction between methods and theses pretty much for granted. It is not, I suggest, quite so unproblematic as he assumes: would one not expect there to be a close connection between the success or failure of a method and the truth or falsity of some rather general thesis? Rescher is not, indeed, quite able to keep this kind of question under the carpet. There are hints of it in his discussion of Hume's problem of the justification of induction; most striking, though, is the opening passage of a chapter on the 'instrumental justification of logic' where Rescher insists—quite rightly, to my mind—that logic is properly to be regarded *both* as a method *and* as a doctrine.

Rescher's discussion of questions raised by the existence of alternative logics is in many ways very shrewd; I found his discussion of the relations between *logica utens* and *logica docens*, and his explanation of the limits of the analogy between logic and geometry and advocacy of an analogy, instead, between logic and grammar, especially interesting. But the ambivalence which results from his concentration on methods while conceding that logic is a matter of doctrine as well as procedure causes some discomfort. At one level, Rescher adopts an instrumentalist stance, declining to ask whether this or that logical system is correct and insisting, instead, that there is a genuinely open choice between alternative logics, a choice to be made on the basis of their success at systematising presystematic reasoning (*logica utens*); and he urges a kind of relativistic pluralism, suggesting that it might be appropriate to choose one logic in one, and another in another application. At the metalogical level, however, he imposes an absolute requirement of truth-preservingness; and this seems to re-introduce the idea of correctness which, in opting for instrumentalism, Rescher seemed to disdain. In the end, then, Rescher's answers to questions about the status of logic seem to me to fall some way short of the heart of the matter; in part, at least, I think, because he pays insufficient attention to two important distinctions: first, the distinction between metaphysical questions (such as: what, if anything, could it mean to speak of a logical system's being correct? and: could there be only one, or might there be several, correct logics?) and epistemological questions (such as: might we be wrong about what we take to be the, or a, correct logic? does fallibilism extend to logic?); and second, the distinction between non-classical logics which add non-classical vocabulary and new theorems and inferences ('extended' logics) and non-classical logics which have just the classical vocabulary but reject some classical theorems or inferences ('deviant' logics).

Rescher attaches epistemological significance to the distinction between the formal and the factual elements in our knowledge; for the pragmatic validation of logic is oblique—turning upon logical systems' success in systematising the *logica utens*—whereas the pragmatic validation of methods of empirical inquiry is direct. So he contrasts his *double-level*, *method-oriented*, instrumentalism with the pragmatic, *wholistic* approach to logical *theses* proposed by Quine in 'Two Dogmas'. The relations between Rescher's and Quine's positions on these issues are, I think, a good deal more complex than Rescher's discussion indicates. For one thing, since Rescher makes at least partial concessions to the doctrinal component in logic, there is not a simple contrast with Quine concentrating on logical theses and Rescher on logical procedures; for another, since Quine's position is developed by contrast with that of the logical positivists, who identified the concept of the analytic (the true in virtue of meaning) and the *a priori* (the verifiable come what may) there is not a simple contrast, either, with Quine rejecting precisely the distinction between formal and factual truth which Rescher accepts.

It is certain, at any rate, that Quine is not, after all, 'the last pragmatist' (Gellner, *The Times Literary Supplement*, 25 July 1975). With this thought-provoking and rewarding book Rescher takes an honourable place in the rich tradition of Peirce and James; and he, let us hope, will be only the latest pragmatist.

SUSAN HAACK
University of Warwick

Brit. J. Phil. Sci. **29** (1978), 189–208 *Printed in Great Britain*

Reviews

FISK, MILTON [1973]: *Nature and Necessity: An Essay in Physical Ontology.* Bloomington and London: Indiana University Press. Pp. 301.

One of Stravinsky's aphorisms applies to philosophy as well as to music: 'To enjoy to the full the conquests of daring, we must demand that it operate in a pitiless light' (Stravinsky [1947], p. 13). Professor Fisk has written a daring book. He argues for the existence of objectively based physical necessities—real necessity, versus conceptual or linguistic. Such necessities, according to Fisk, require natures as their basis, where natures are components of things but not distinct from them. He then uses his resulting ontology of complexes, plus corollary views on dependence and sameness of entities, to give original and often provocative solutions to a number of deep problems ranging from analyticity to relations, action, time, events, capacities, natural kinds, and causality, among others. Throughout, a guiding aim is 'to break out of [the] cycle of dualism and reductionism' which he believes unavoidable (p. xii) on what he considers the received view (largely Humean) of dependence and sameness, and on received ontologies of simples.

As Fisk says, 'a work with such broad scope is inevitably programmatic' (p. xiii). I have no objection to its being so programmatic. On the contrary, the ontology's potential for illuminating in new ways such a variety of old problems is a strong consideration in its favour—or *would* be, if alternative ontologies were not arguably of *equal* potential, certainly according to their proponents (including those like Goodman who claim to be able to do without ontology altogether.) Thus in view of the arguably equal power of the alternatives, if we are to enjoy the conquests of Fisk's daring, we must examine his basic arguments 'in a pitiless light.' Not that the burden of proof necessarily is on Fisk. Indeed his book by itself might succeed in showing that the burden is on opponents of mind-independent necessity. But whether it *does* succeed depends not only on its holistic explanatory power (with regard to relations, action, time, causality, *etc.*), but also on the soundness of its particular basic arguments.

I shall concentrate on one: the argument for objectively based necessities, which is expressly crucial to the rest of the book (p. 3). I do so with considerable misgiving, since the fascinating web of Fisk's position as a whole can hardly be appreciated by focusing so narrowly on one of its anchors, however crucial. But webs do have anchors, some with many more strands running from them than from others, and we must determine what load they can bear.

Fisk's strategy is to argue from the indispensability (p. 76) of a certain practice, to something's being a genuine part of reality (p. 30): 'The practice of action based on prior experience is incompatible with the view that there are no physical necessities . . . whose necessity is objectively based' (p. 24). He is, of course, aware of the pitfalls of any such 'presuppositional' argument (p. 76). For example, he tries to show how, despite our misgivings, it is *not* 'entirely possible that the world is other than what it is made to seem by the practice of acting on the basis of prior experience' (p. 19). He tries to show this by claiming the practice is so fundamental that without it, 'there would be no human practice at all' (p. 75), and then to argue:

> . . . to affirm its basic role is to grant the beliefs about the world that are made undeniable by this practice. Nothing in the vast cultural superstructure of practices dependent on it can make legitimate the possibility of conditions conflicting with those beliefs (p. 19). . . . The possibility of a conflict between the world and a basic practice cannot be justified (p. 20).

The passage on page 19 is followed immediately by one in which Fisk tries to hoist his opponent with a dilemma (which fails, I believe, for fundamentally the same reasons (1)–(4) do, below). So far as I can determine, these passages represent Fisk's only complete response to the kind of objection they treat, namely that however fundamental a practice, inferences from it to the world are suspect (*cf.* pp. 18–19). Unfortunately Fisk's response is inadequate.

To being with, underlying all the elaboration, including what occurs later in the book, Fisk's indispensability argument has the form:

(1) We need to achieve ends E (*e.g.* predicting, communicating, propositional thought, social regimentation, ceremony, art, and so on; *cf.* pp. 9–10).
(2) To achieve E, practice P is indispensable (*e.g.* basing action on prior experience).
(3) P presupposes that condition C exists (*e.g.* objectively based necessity).
(4) Therefore, C exists.

Even if all of (1)–(3) could be established, (4) would not follow (nor do (1)–(3) make (4) more likely than some alternative, as will become clear below). What would follow is only that we need to *presuppose* that C exists. At this point someone might ask, 'How could we say, defensibly, that we need to presuppose C's existence—and not do so?' Bas van Fraassen's reply to this sort of query about indispensability arguments strikes me as sufficient:

> Such problems will be recognized, this late in the day, as philosophically spurious, though existentially excruciating. The situation is exactly that of the nineteenth century clergyman who is losing his faith. In his Ibsen world it is unquestionably accepted that life is impossible without faith [premise of form (2)], that the existence of God alone can ground morality and give meaning to life [premise of form (3)]. But how can he make the inference to the existence of God [to (4)]? Following our philosophical conscience we must counsel suicide sooner than an invalid inference. (Van Fraassen [1975], p. 734)

Invalidity of the inference aside, I have questions about Fisk's premises, and about his whole approach.

His approach aims to establish *objectively* based necessities. But a necessary condition for such objectivity, roughly speaking, is invariance through perspectives, frames of reference, coordinate systems, and so on. (Fisk himself speaks (in effect) of invariance through 'switch of conventions or perspectives' (201).) That is, we are willing to grant objective, observer-independent existence to something only if it corresponds to an 'invariant'. Physicists and philosophers alike are fond of asking, in the spirit of a thought-experiment, what a Martian would see or say who was in rapid motion relative to our frame, or who was very differently endowed with respect to sense receptors, nervous system, language, science, conceptual scheme, and so on. Typically the point of such fictions is to vary the perspective, and to challenge our almost ineradicable anthropocentricism, of which unsuspected residues frequently frustrate our search for the real—that is, for the 'invariant'—as physicists and some philosophers well know.

Yet Fisk's whole approach begins with an avowedly anthropocentric notion, that of the practice of basing action on prior experience (he talks always of *human* practices). How likely is it that we would establish an *objective* feature of the universe from such a beginning? Not very likely, I should say, though perhaps not impossible either. In any event, in view of such an unpromising strategy, we should not be surprised to find severe trouble with his premises. Most philosophers are likely to wonder about premise (3)—in effect, whether induction presupposes necessity—but I am equally concerned with premise (2), the alleged indispensability of basing action on prior experience. Not only hypothetical Martians but at least some earthlings would not find it indispensable, or so I shall argue. But first we must find out exactly *what* it is about this practice, according to Fisk, that is *both* indispensable *and* presupposes an objectively-based necessity.

The crux is that 'even non-judgmental action on prior experience requires that there be support, from such experience, for the proposition that the project will succeed' (p. 13). In particular, it is only when the data e about the prior experience *increase* support for the *truth* (pp. 78–80) of the proposition or hypothesis h on which one acts that necessity is involved—*i.e.* only when $p\,(h/e\,.\,f) > p(h/f)$, where f records the background features (pp. 80–5). 'Support for hypotheses', meaning increased support in the above sense, 'and an ontology that excludes necessities form an incoherent pair' (pp. 79, 85).

In sections 3–5 of chapter 4, Fisk explains why objective necessity is involved when data increase support—in effect, why induction presupposes necessity (premise (3)). His argument is ingenious, complex, and nicely dissected of late by Richard Rorty (Rorty [1976], *esp.* pp. 349–50) who argues correctly, I believe, that these sections fail of their purpose—in particular, that Fisk begs the question against his Humean opponents. Because I have little to add to Rorty's treatment, I shall concentrate instead on premise (2), the alleged indispensability of basing action on prior experience.

In particular, my concern is with Fisk's *assumption that the (notion of) support of the truth of a hypothesis (p. 78) is indispensable for (human) activity, or that 'support is something that a . . . project should have in order to be . . . undertaken'* (p. 12). My concern is not reduced in the least by his allowing that there are very different rules of induction from which the degree of support of a hypothesis by data might be inferred, or by his allowing that *theorising* might proceed on a purely contemplative level, unlike other, more practical occupations in science. Nor is it reduced by his allowance for abductive inference or inference to the best explanation (which he claims exists, like all inferential practice, only in a context in which the ground already has been prepared by the support of other hypotheses by data (pp. 77, 80)). My objection, I think, is deeper and more urgent.

Enter the Martian, named 'Sirkrp'. Imagine that Sirkrp and his kind act only on hypotheses they accept in the following way. Their brains are (or contain) super-fast hypothesis generators: in the time it takes a human to think of a hypothesis relevant to a given problem, Sirkrp scans thousands. Sirkrp is not particularly attached to any of these hypotheses, which is fortunate since his brain is also super-fast in rejecting any that are contradicted by his data, the latter also collected super-fast, with an eye (or an antenna) toward demolishing hypotheses. That is, he tends to filter out positive data (*i.e.* positive instances of hypotheses, which may or may not increase their support) and stores the dissonant data, just the opposite of the average human, of whom numerous empirical studies show that his brain tends very strongly to filter out the dissonant.

Suppose further that Sirkrp acts only on those hypotheses that survive this super-fast generation-rejection process. In case more than one still stands at the time when action they are relevant to is required, he selects among the survivors according to *his* quality space, and/or his sense of natural kinds, simplicity, generality, coherence, methodological conservatism, or whatever. When all else fails and action is required, he flips a coin.

At no point does Sirkrp assume the *truth* of any hypothesis he acts on, or even that it is more *probably* true than some other hypothesis. Instead he and his kind have evolved so as to find it natural to the point of irresistibility to reflect that whereas the rejected hypotheses are known to be false relative to the data at hand (or at tentacle), the survivors at least are not known to be false, so one had better act on them *faute de mieux*. Something about the Martian's harsh environment and evolution determines him to think always in terms of choosing the lesser of evils, and to have no very firm expectations.

How might Fisk (or anyone else) object to this bit of science-fiction, this thought-experiment? It might be objected that to act on a hypothesis is to presuppose that it is true, if only because action implies some expectations about the future. The trouble with this objection is that we earthlings ourselves act rather often on hypotheses we not only do not presuppose are true, but believe are false. For example, the hypotheses (equations) engineers actually use to calculate space-craft trajectories are Newtonian. According to relativity theory, strictly speaking they are false. Yet life and fortune are committed to the success of the Newtonian predictions.

It might be replied that what we are *really* doing is committing ourselves to the *approximate* success of the predictions, and to the truth of the Newtonian hypotheses, *to within the degree of accuracy required* to land men on the moon. I quite agree that very often this is what we are really doing in acting on a hypothesis. But it is irrelevant to the question of whether in acting on a hypothesis we must presuppose it is *true*. For truth *simpliciter* is one thing, and *truth to within the degree of accuracy required for practical action is quite another.* Thus even if practical action implied expectations about the future—and I'm not sure it always does—the expectations need be fulfilled only to within the degree of accuracy required by the practical action at hand. By the same token, it need only be *probable* to an extent appropriate for the action at hand that the expectations will be fulfilled to within such accuracy. In either case, it would not follow that to act on a hypothesis is to presuppose that it is strictly true. So far from there being any warrant for projecting our alleged inductive psychology and/or conceptual scheme into the Martian mind, there is some question about projecting it into our own, even where (or perhaps especially where) practical action is involved, as opposed to contemplative theorising.

However, Fisk distinguishes *belief* and *acceptance* from *support*; and his argument for objectively based necessities requires only that *in fact* the data increase support of the truth of the hypothesis on which one acts, and not that one *accept* it in light of the data, or even that one would be *warranted* in accepting it (since 'the degree to which data may support an hypothesis may be too small to warrant its acceptance' (pp. 80–1)). Consequently Fisk would be more likely to object to our bit of science-fiction that *whether Sirkrp and his kind realise it or not*, in accepting and acting on hypotheses that survive their super-fast selection process they *are* acting on hypotheses for which the data increase support of their truth. 'Whatever the full requirements of acceptability, they will include the minimal requirement of support' (p. 81).

What argument could be given for the claim that Sirkrp's data do after all increase support for the truth of the hypotheses he acts on? There is a fundamental difficulty with any such argument. Recall that Sirkrp filters out positive data and stores dissonant. Now it is possible for Sirkrp to accept some hypothesis *h* for which he has (and has seen) *no* positive data (no positive instances of *h*), only data that conflict with the alternatives to *h* he happens to have generated so far. How would Fisk propose arguing that the data which negate *h*'s alternatives increase support for the truth of *h*? Coming from an inductivist, any such proposal would be supremely ironic, since inductivists find Popperian and other "eliminativist" theories wanting precisely on the grounds that mere negation of alternatives does not increase support for the truth or the probability of *h*.

What about situations in which Sirkrp does have (or has seen) some positive data for *h*, in addition to data that contradict all the generated alternatives? Even then it would not follow that Sirkrp's data increase support for the truth or likelihood of *h*. The mere fact that he has some positive data does not imply that the data (either those data or *all* his data) increase support for *h*, or even that they increase the probability that *h* is true by comparison with *h*'s negated alternatives. *The implication would not hold even if his positive data were collected in a wide variety of carefully controlled experiments in different circumstances, and even if they resulted from his best efforts to demolish h.*

Perhaps the most interesting reason why the implication would not hold makes use of an inductive inference. We are free to use inductive inferences here because of our science-fictional attitude toward the Martians. That is, we have *not* assumed that the Martian way is right or corresponds to the way the world is (whatever that would mean), or that it is the correct method of science, or anything else about them that would entail earthlings ought not use inductive inferences. I have left that issue open so as not to beg crucial questions against Fisk. In arguing *against* the alleged indispensability of induction I am *not* arguing *for* the Martian way. I am instead arguing merely for the physical possibility of such creatures, in order to show that the practice of acting on hypotheses whose truth is supported by the data is neither indispensable nor invariant through alien intelligence, conceptual schemes, frames of reference, or whatever, in any way that would entitle us to suspect it corresponds to some mind-independent feature of the universe, whether that feature is objectively based physical necessity or anything else.

Now suppose that *every* time in the past when Sirkrp has accepted some hypothesis h, including any for which he had positive data, sooner or later h turned out to be *false* (not only relative to data gathered by him subsequent to his accepting h, but more importantly, relative to all *our* data as well). Suppose further that his bad luck is well known by Sirkrp and is part of his data. Thus, it is part of his data that ***every past instance of positive data collected in his way for any h he accepted has been an instance of data for false h.*** Then we would have sufficient inductive grounds (though Sirkrp would spurn them) for concluding not only that the *next* hypothesis h' Sirkrp accepts in this way is false, but that any positive data *he* has for h' neither support the truth of h' nor increase the probability that h' is true by comparison with its rejected alternatives. On the contrary, Sirkrp's universally bad luck should lead us (though not him) inductively to suppose that his having positive data for h' *decreases* the probability that h' is true. But even if the probability that h is true were not decreased, nevertheless the mere fact that Sirkrp has positive data for h'—even a great deal, collected in very varied circumstances—would not imply that his data increase support for its truth. For *his* data include the datum *we* used to argue inductively both that h' is false and that his present positive data for h' do not increase its support, namely the datum that his every past instance of positive data for any h he accepted has been for false h.

In Fisk's terminology (pp. 81–5) my argument is this. Suppose proposition e records all Sirkrp's positive data for h', and f records all other data he has at the time of accepting h'. According to Fisk, e increases support for h' only if $p(h'/e\,.f) > p(h'/f)$. But I claim that $p(h/e\,.f) = 0$. For f includes the datum that every past instance of positive data collected by Sirkrp for a hypothesis is an instance of data for a false hypothesis. Hence the probability that the next hypothesis h' is true, given both the next instance e of Sirkrp's positive data and f, is zero, just as the probability that the next sample of water will freeze is zero, given both that it is heated to 100°C, and that in every past instance of being heated to 100°C water has boiled. Since $p(h'/e\,.f) = 0$, $p(h'/e\,.f) \ngtr p(h'/f)$, from which it follows that e does not increase support for h'.

It might be held that no creature would long survive whose *every* accepted hypothesis has been false. After all, he must be right *some* of the time when he hypothesises, 'Food here now' or 'Food over there moving that way'. But recall that truth is one thing, and truth to within the degree of probability required for success in practical action is another. Now suppose that every predicate in Sirkrp's language is satisfied only by objects that have determinate position and momentum simultaneously. Suppose further that (unbeknownst to Sirkrp) quantum indeterminacy holds not only for micro-objects but (as many earthling physicists believe) for macro-objects as well. Then no macro-objects satisfy any of his predicates. So hypotheses like 'Food over there moving that way' would not be strictly true: the food does not have determinate simultaneous position and momentum (similarly for its energy, and any other complementary variables). But the food has them nearly enough for Sirkrp to eat quite well, since the uncertainties that would affect ordinary macro-objects would be extremely small—indeed, 'negligible for all practical purposes', as one says.

In this way an intelligent creature could survive handsomely whose accepted hypotheses have all been false, including those hypotheses about his fellow creatures he needs for communicating, plus those needed for 'propositional thought . . . social regimentation, discipline . . . punishment . . . ceremony and art' (pp. 9–10). Sirkrp's culture would not be impoverished by action on hypotheses that are not supported in the way Fisk thinks they must be. Hence, induction, or the practice of basing action on prior experience in the sense Fisk intends, is not indispensable or basic in the sense he requires, namely that without it, all other practices would be 'difficult to conceive' (p. 8), 'left in the air' (p. 19), or non-existent (p. 76). Therefore premise (2) is false. Nor is the practice invariant in any sense that would entitle us to suspect it corresponds to an objective feature of the world. Fisk's major goal was to extend scientific realism to include necessities (and thereby natures), but the cause of scientific realism is ill-served by the anthropocentrism of his argument.

JOHN F. POST
Vanderbilt University

REFERENCES

FRAASSEN, B. C. VAN [1975]: 'Critical Notice of Hilary Putnam, *Philosophy of Logic*', *Canadian Journal of Philosophy*, 4.
RORTY, R. [1976]: 'Necessity and Realism: Milton Fisk's *Nature and Necessity*', *Nous*, 10, pp. 345–53.
STRAVINSKY, I. [1947]: *Poetics of Music: In the Form of Six Lessons*. Cambridge, Mass.: Harvard University Press.

GRENE, MARJORIE [1974]: *The Understanding of Nature: Essays in the Philosophy of Biology*. Dordrecht: D. Reidel. $32.50 Pp. viii+374.

These essays by Professor Grene were written over a span of twenty-eight years. Not surprisingly, on various of the topics discussed she has changed her mind, but she has continued to believe that there is a positive relation between metaphysics and science, and that biology not only is one science where this relationship can be explored with profit, but also that it gives rise in a unique way to what ought to be central philosophical problems. She has remained a philosophical realist and an implacable opponent of what she variously names as atomism, mechanism, reductionism and logical empiricism and their manifestations in certain biological theories. Although she too often relies on near caricature or surprisingly feeble versions of these positions, a sympathetic reader can supplement or amend her accounts without always destroying the relevance of her criticisms. Aristotle, as opposed to Aristotelianism, has been a major influence and in 'Aristotle and Modern Biology' she argues persuasively that in studying the conceptual foundations of biology we still have much to learn from his discussions of *telos*, *eidos* and 'the-being-what-it-is'. She has also been strongly influenced by certain phenomenologists and Polanyi, though she is not uncritical of them.

These influences show themselves most clearly in her attacks on reductionism which, like her other enemies, assumes a one-levelled ontology, a term she never explains adequately. It either implies or is implied by the thesis that a whole can be explained in terms of its parts alone. She deploys three main arguments to show that a satisfactory account of the explanatory nature of biological theories and the characteristic differences between living and non-living matter requires a many-levelled ontology. The first is derived from Aristotle. All matter is informed, so not only is a knowledge of something's form (what it is or what it does) required to understand how it comes to be or how it works, a claim which few, if any, would deny, but also there is a sense in which form determines matter. Where the form of an organism is given by the biological laws relevant to it, physico-chemical theories about the workings of its molecular parts cannot account alone for the biological phenomena. In chapters 3 and 4 and elsewhere she gives an argument from Polanyi to support this. According to theories in physics and chemistry any arrangement of the four bases of DNA is equiprobable and those theories cannot explain why a particular arrangement exists. And yet the growth of the organism depends upon a set of particular arrangements. I think she wants to say that it exists because it is the kind of organism it is. But the 'because' is not causal and apart from suggestions that we have here a formal explanation the idea is not developed further. She does not move convincingly from a methodological point to an ontological claim. Her third argument is that reductionism is incoherent. That people believe the theory is wholly determined, causally, according to the theory itself and so a reductionist has no grounds for distinguishing between truth and falsity. As a version of an argument against determinism it has its attractions but difficulties too.

Darwinism is seen as emerging from an atomist or mechanist tradition. Its ruling passion is to explain away structure which must be reduced to the conditions out of which it arose rather than be acknowledged as structure in itself (p. 141). In 'Two Evolutionary Theories' where she looks at the arguments of Simpson and Schindewolf as providing a paradigm of scientific controversy, and 'Statistics and Selection', she is critical of both the neo-Darwinian and Fisher's genetic theories, the latter especially for its ambiguous use of 'improvement'. Her views on the neo-Darwinian theory have changed, as she acknowledges in the preface, and in 'Explanation and Evolution' she argues, against Ayala, that it is not a teleological but causal theory. Although it can explain how 'telic phenomena' came into existence, no teleological concepts can be employed in any legitimate version of the theory, because one cannot explain *how* something came into existence by relating it to something not yet in existence (p. 223), a point she also argues in her discussion of Aristotle's notion of *telos* (p. 84). In this essay, she finds no place for teleological explanations in biology as opposed to teleological descriptions of phenomena which require explanation.

Throughout the essays on Aristotle, teleology, evolution and reductionism, there are arguments or claims about the uniqueness of biology. Although she alludes to the arguments of Bertalanffy and Elsasser, the one she favours from Polanyi, mentioned above, does not by itself establish that biology is unique. In three long essays on Portmann, Straus and Plessner she discusses very different attempts to show that living matter is radically different from non-living. She sets out to explain their views to those not familiar with the tradition in which they wrote. They are important because they provide us with the basis of a philosophical anthropology which will take us away from a Cartesian view of man and consequently 'an empty idealism or an absurd reductionism'. But their importance cannot be assessed until the central concepts they employ can be explained. For example, the concept of positionality, the way an organism bounds itself, plays a crucial role in Plessner's theory. The boundary of a non-living thing is where it stops and is identical with its contour or outline. The boundary of a living thing 'is a part of itself, not merely a virtual in-between between body and medium' (p. 324). While she acknowledges the difficulty of explaining this distinction, what she says does not make it less obscure.

Causality, perception, the difference between man and brute and man's relation to the natural world are among the other topics covered in these essays. They all exhibit Professor Grene's enthusiasm for philosophy, both its perennial problems and its relevance to modern biology. Her readers might wish for more patient and careful accounts of the positions she attacks and her own arguments, but her essays may at the very least be taken as worthwhile invitations to think about the problems she presents.

DAVID HIRSCHMANN
University of Bristol

THOMASON, RICHMOND H. (*ed.*) [1974]: *Formal Philosophy: Selected Papers of Richard Montague.* New Haven and London: Yale University Press. Pp. 369.

The volume is a posthumous collection of Richard Montague's philosophical papers, with an excellent introduction by the editor Richmond H. Thomason.

In my review I shall confine myself to the papers which are relevant to linguistic problems. This includes them all, except the paper 'Deterministic Theories' which makes up the last chapter of the book.

Although from a technical standpoint Montague's papers are quite sophisticated, the basic assumptions of his approach to linguistic problems can be presented relatively easily. Let me briefly discuss them.

The main objective of any semantic considerations concerning a language $\mathscr{L}$ (regardless of whether it is artificial or natural) is to define the notion of truth for that language. In order to do so one has:

(1) to specify the syntactical rules which determine the formation of compound expressions of the language, and in particular the rules governing the formation of formulas (a sentence is taken to be a formula without free variables);

(2) to interpret the non-logical constants in the vocabulary (the set of elementary expressions) of the language;

(3) to state truth conditions which taken together form a recursive truth definition for the language.

This programme is no doubt familiar to any reader acquainted with the basic ideas of logical semantics. The novelty of Montague's approach lies in his definition of an interpretation. He assumes that in order to provide a semantic interpretation $\mathfrak{A}$ for a language $\mathscr{L}$ one has to specify three elements:

I: the set of all possible worlds;
U: the set of all possible individuals;

F: the interpretation function for $\mathfrak{A}$ which, for each possible world i in I, given any non-logical constant C of $\mathcal{L}$, determines the ***extension*** (***denotation***) C^i (Montague writes $F_C(i)$) of C with respect to i.

Thus $\mathfrak{A}$ may be conceived as the triple $\langle I, U, F\rangle$.

The function $\{C^i\}_{i\in I}$ (*i.e.* the function which to each i in I assigns the extension C^i of C at i) is referred to by Montague in his informal comments (*cf. e.g.* p. 98 or *p.* 157) as the ***intension*** (or ***meaning***) of C. Thus F defines both the meaning and the denotation of the constant symbols of $\mathcal{L}$.

Truth-conditions for $\mathcal{L}$ are to determine the truth value of sentences of $\mathcal{L}$ relative to a given interpretation $\mathfrak{A}$ and to a possible world i chosen from the set of all possible worlds of $\mathfrak{A}$. Thus the objective of semantics is to define for each sentence ϕ of the language in question the statement 'ϕ is true at i under $\mathfrak{A}$'.

The conditions which Montague imposes on F and the truth-conditions which he formulates for the languages he discusses are by no means all obvious. At least some of them appear to have no clear intuitive justification.

In order to illustrate the point compare *e.g.* the conditions imposed on extensions of predicate constants.

If P is a first order n-place predicate symbol, then for each $i \in I$ Montague defines P^i to be an n-place relation defined on U, in symbols $P^i \subseteq U^n$. (Incidentally this way of interpreting P is analogous to that accepted for individual constants. If a is such a constant, then a^i is assumed to be an element of U.)

Suppose now that P is a second order predicate, *i.e.* some of the arguments of P may be first order predicate symbols.

Define a predicate symbol P to be of type $\langle s_1, \ldots, s_n\rangle$ (where each s_j is an integer $\geqslant -1$) if and only if the atomic formulas constructed by means of P are postulated to be of the form $P(\xi_1, \ldots, \xi_n)$ where ξ_j is either an individual symbol, if $s_j = -1$, or a first order n-place predicate symbol, if $s_j > -1$. (Note that $\mathcal{L}$ may involve 0-place predicates; each such a predicate P is at the same time a sentence of $\mathcal{L}$. Observe also that each first order predicate is of type $\langle -1, -1, \ldots, -1\rangle$ with the number of -1's being $\geqslant 0$).

Put $U^{-1} = U$ and $U^0 = 2^{\{\wedge\}}$, where $\wedge$ is the empty set. By analogy with the condition for first order predicates it would be natural to expect that for each predicate constant P of type $\langle s_1, \ldots, s_n\rangle$, $P_i \subseteq U^{s_1}\times \ldots \times U^{s_n}$. In fact, Montague imposes a considerably more complicated condition on P^i. Namely he demands (*cf.* p. 130 and 157) that:

(*) If P is a predicate constant of type $\langle s_1, \ldots, s_n\rangle$, then $P^i \subseteq V(s_1)\times \ldots \times V(s_n)$, where $V(s_j)\colon I \rightarrow 2^{U^{s_j}}$ whenever $s_j > -1$ or else $V(s_j) = U$.

$2^{U^{s_j}}$ is the set of all subsets of U^{s_j}, *i.e.* the set of all s_j-place relations defined on U. Thus $V(s_j)\colon I \rightarrow 2^{U^{s_j}}$ is the function which maps I into the set of s_j-place relations. Such a function is called by Montague s_j-place *I-predicate of members of U*.

Note that intensions of first-order predicates are I-predicates. Thus the denotation P^i of P is a relation of which the relata are extensions of the arguments of P, when the latter are individual constants, and their intensions otherwise.

Since an atomic sentence $P(\xi_1, \ldots, \xi_n)$ formed by means of an n-place predicate P is defined to be true at i under $\mathfrak{A}$ if and only if $P^i(\xi_1^i, \ldots, \xi_n^i)$, it turns out then that even the truth value of atomic sentences may depend both on extensions and intensions of the terms a sentence involves.

The double way of treating predicates has its analogue in the way in which quantifiers are treated. If ξ is an individual variable, the expression 'There is ξ such that...' is understood as 'There exists a possible object ξ in U such that...'. But if ξ is a predicate variable, 'There is ξ such that...' is meant to abbreviate 'there exists an I-predicate over U such that...'. Thus, in contradistinction to first-order quantification which is extensional, second-order quantification is intensional.

Montague's decision to treat second order predicates as intensional is, I would guess, motivated by the fact that the predicates of ordinary language in which he is especially interested are (or at least seem to be) of that sort. For instance, epistemic predicates such as *e.g.* 'believes that', 'knows that' may be treated as predicates of type $\langle -1, 0\rangle$. The sentence 'John believes that ϕ' may be interpreted (*cf.* p. 139) as expressing a relation which John bears to the proposition 'that ϕ'. Now the proposition 'that ϕ' is formalised as $\mathrm{T}\xi\Box(\xi \leftrightarrow \phi)$, where ξ is a 0-place predicate variable, and T is a symbol definable in terms of intensional languages which reads 'the unique ξ such that'. The expression $\mathrm{T}\xi\Box(\xi \leftrightarrow \phi)$ (and thus its ordinary language counterpart 'that ϕ') decoded with the help of assumptions which determine the way of interpreting the symbols T, $\Box$, $\leftrightarrow$ (truth-conditions for truth functional connectives, in particular for $\leftrightarrow$, are standard; $\Box$ is postulated to be true at i under $\mathfrak{A}$ whenever ϕ is true at each j in I under $\mathfrak{A}$), turns out to be the function which to each i in I assigns either $\{\wedge\}$ or $\wedge$, depending on whether ϕ is true or false at i.

The intensional treatment of second order predicates also seems to conform well with the meaning of such words of ordinary language as *e.g.* 'owns', 'seeks', 'seems', 'rises'. A detailed discussion of the semantic status of these words (and some others whose semantic status cannot be adequately accounted for within extensional logic) has been carried out by Montague in 'On the Nature of Certain Philosophical Entities' (ch. 5).

The purely semantical approach does not suffice to account for certain peculiarities of ordinary languages and must be supplemented by a pragmatic approach. To provide a pragmatical (in Montague's sense) interpretation for a language $\mathscr{L}$, one must define the way in which the truth value of sentences of $\mathscr{L}$ depends on the contexts of usage of those sentences (the person speaking, the time at which the sentence is uttered, *etc.*).

The same technique which is applied to account for how the truth value of a sentence depends on a particular selection of a possible world can be applied to account for how it depends on the context of use of that sentence. Thus given an interpretation $\langle I, U, F\rangle$ we may assume that I consists of instants of time and postulate, for example, that a sentence $\mathscr{P}\phi$, where $\mathscr{P}$ is a one-place operator (connective), is true at i under $\mathfrak{A}$ if and only if ϕ is true at some j in I earlier than i. Under the interpretation proposed it is appropriate to read $\mathscr{P}\phi$ as 'it has been the case that ϕ'.

In an obvious way one may define the truth conditions for personal pronouns (for instance in the case of the pronoun 'I' the set of all contexts of use should be defined to comprise all possible speakers). A similar technique helps to analyse demonstrative pronouns. The reader can easily suggest some further applications of the pragmatical approach.

If necessary, elements of I may be defined to be compound objects of the form $\langle i_1, \ldots, i_k\rangle$, where i_1 represents say a possible world, i_2 an instant, i_3 a speaker *etc.* Obviously this is one way in which semantical and pragmatical approaches can be combined.

Although in my exposition I have tried to avoid going into the details of Montague's approach, I hope that even the uninitiated reader has been able to grasp its most characteristic features.

In dealing with natural languages Montagu's semiotical ideas can be applied in two-fold manner.

The first possibility consists in defining formal languages intended to serve as explanatory models for certain parts of a natural language (for a discussion of the notion of an explanatory model *cf.* Hintikka [1969]). This course is followed by Montague in the papers forming chapters 3–5 of the book, where he defines pragmatical and intensional languages and discusses briefly some ways of strengthening the latter.

Instead of an indirect approach one may try a direct attack on those semiotical problems relevant for natural languages. After all, it seems perfectly possible that the truth values of sentences of natural languages depend on the meaning of the terms from which the sentence is built and the way in which they are combined together. If this conjecture is true or at least true for a certain large and sufficiently interesting fragment of a natural language, then it is of great importance to decide what is to be meant by the meaning of a term, *i.e.* to decide what kind of semantics (and pragmatics) suits the purpose of an analysis of a natural language. The answer to this question offered by Montague is: exactly the same kind of semantics as that applied in dealing with formal languages. The first sentence of his 'English as Formal Language' (ch. 6) reads: 'I reject the contention that an important theoretical difference exists between formal and natural languages', and in 'Universal Grammar' (ch. 7) again at the very beginning of the paper—he repeats: 'There is in my opinion no important theoretical difference between natural languages and the artificial languages of logicians; indeed I consider it possible to comprehend the syntax and semantics of both kinds of languages within a single natural and mathematically precise theory'.

The problems arising when a natural language becomes an object of direct semiotic study are of very specific nature. As Montague has pointed out, 'there will often be many ways of syntactically generating a given set of sentences, but only few of them will have semantic relevance' (p. 210). The syntactical rules of forming compound expressions should be selected so that they are adequate in the sense that they generate all expressions of the language in question and nothing more; but furthermore they have to conform to the semantic and pragmatic role of expressions. The syntactic rules should allow us not only to reconstruct the way in which a compound expression was built but to grasp the meaning of the compound expression from the meaning of its components.

On the other hand Montague's remarks concerning the many ways of syntactically generating a given set of sentences should be supplemented by the remark that there are many ways in which the semantic function (category) of terms of a natural language can be defined. To illustrate this remark by an example let me mention Montague's decision (*cf.* ch. 8) to treat proper names as expressing properties of properties. The expected interpretation under which a proper name denotes an individual object would be possible provided that the semantic role of other terms was redefined. As Montague puts it 'the constructing of syntax and semantics must proceed hand in hand'.

There is one specific difficulty, which must be overcome before the programme of setting up a linguistics designed to be applicable equally to formal and to natural languages can be carried out. It is the difficulty which stems from the ambiguity of sentences of natural languages.

It would not be easy to give a short and, at the same time, adequate account of how Montague copes with these particular difficulties that must be overcome before one can give a rigorous theory of the syntactic, semantic and pragmatic features of natural languages within the general framework he has developed in his studies of formal languages. At the same time it is not really essential to know the details of his approach in order to appreciate its novelty.

Montague was the first to realise that the linguistic problems of natural languages should be examined within a certain fixed semantic framework. This framework should be set up before we begin to analyse natural languages. Certainly, it may turn out that certain properties of a natural language under investigation cannot be accounted for within that framework and that the latter must be modified. Perhaps the framework proposed by Montague (whose central notion, that of an interpretation, we have discussed) is not the best one. In that case Montague's approach must be improved, (in particular his notion of an interpretation should be reconsidered). But, I am afraid there is no alternative to the basis of his approach namely, the thesis that a sufficiently precise and general semantic framework is indispensable in linguistic considerations if their outcome is to be of any considerable value.

The reader who is interested in linguistic problems will find the book extremely rewarding. For those who might be discouraged by Montague's excessively pedantic style I recommend Thomason's introduction which will facilitate an understanding of Montague's ideas.

RYSZARD WÓJCICKI
Institute of Philosophy and Sociology,
Polish Academy of Sciences

REFERENCE

HINTIKKA, K. J. J. [1969]: 'Epistemic Logic and the Methods of Philosophical Analysis' in *Models for Modalities*. Dordrecht: D. Reidel.

ACKERMANN, ROBERT J. [1976]: *The Philosophy of Karl Popper*. Massachusetts: University of Massachusetts Press. $15. Pp. 212.
JOHANSSON, INGVAR [1975]: *A Critique of Karl Popper's Methodology*. Stockholm: Scandinavian University Books. n.p. Pp. 210.

Ackermann sets out to give a comprehensive exposition and critical appraisal of Popper's work and a guide to some of the main related literature. With the exposition he is quite successful. His general appraisal is that Popper's achievements, while being such as to call for further work, are impressive; but on specific issues the praise is sometimes faint and the criticism discretely severe. Thus, on statistical inference and quantum physics, Ackermann holds that Popper has not shown his approach to be superior to rival approaches but has established only that the work of statisticians and physicists is compatible with his methodological views. In connection with the social sciences, he argues that Popper is too anti-psychologistic, does not do justice to Marx and Freud, cannot completely account for structural factors in social change, and has some explaining to do if the world 3 doctrine is to square with methodological individualism; he suggests, further, that the theory of the open society underestimates the problems of free communication and criticism in a large industrial society. The book begins with three chapters devoted to distinguishing normative from descriptive tendencies in methodology, briskly expounding logical positivism, characterising Popper's general methodological position in opposition to subjectivism (or relativism) and positivistic inductivism, and suggesting how that position might be developed to become a more thoroughly evolutionary one. The subjects of the remaining six chapters are: probability, verisimilitude and corroboration, Popper versus Carnap on induction, physical indeterminism, quantum theory, and the methodology of the social sciences and social philosophy.

Much of the book consists of routine—but, for students, helpfully informative—exposition, and the author's own contributions are for the most part unobtrusive. The most striking one is his proposal to correct and develop the evolutionary metaphor in such a way as to 'reconcile viable features of positivism with viable features of subjectivism in a Popperian hybrid' (p. 61). If we turn the metaphor into an explicit analogy, Ackermann's basic suggestion is that the analogue of a *theory* should be not an individual organism but a *species*, an *environment* being the analogue of a *data domain* (defined on p. 60 as 'a range of data that can be gathered by certain methods'). Species fitness involves the harbouring of ill-adapted individuals which may flourish in the event of moderate environmental change; and different species in the same physical area may non-competitively occupy different ecological niches. Analogously, a theory is to be thought of as existing in a variety of versions (variety being preserved by imprecision in our language) and thus being flexible enough to survive some changes in the data domain being produced by new techniques; and Newton's theory continues to appear in science textbooks because it is 'well adapted to data domains defined basically by naked-eye observations assisted by the low-power light telescope and various other pieces of classical apparatus' (p. 63).

Ackermann claims for this proposal that it 'has the advantages of theoretical diversity as urged by Feyerabend and Kuhn while attempting to preserve objectivity in terms of theoretical fit to data domains' (p. 62). This apparently instrumentalist objectivity would not be enough for Popper. For one thing, nothing remains here of the idea of possible long-term *progress* in the direction of objective truth. But Ackermann regards that idea as a dispensable part of the positivist legacy: in using the notion of verisimilitude, Popper allows the 'picture of factual accumulation to reassert itself from positivism in order to combat subjectivism' (p. 92). For another thing, the proposed view appears to involve no distinction between what a theory says about the *world* and what it says about the *phenomena*. But, again, Ackermann questions Popper's realism and the view that the truths expressible in one language are translatable into any other (enriched if necessary). And, taking his cue from Kuhn, he remarks: 'Instead of saying that there is one world with different descriptions, we can say that there is a physical universe containing potential stimuli giving rise to different worlds as man interacts with the universe with different cognitive schemes and varied interests' (p. 53). Two points which remain unclear to me are whether Ackermann is suggesting that there are (at least) as many worlds as data domains and whether he is recommending that we should stop trying to find out about the physical universe. A footnote gives notice of a more extended treatment to come.

Although similar in length to Ackermann's, Johansson's book is narrower in scope and examines Popper's arguments on methodological questions in greater detail. Its general thesis is that harmony between philosophy of science and philosophy of perception, which obtained in logical positivism, is to be restored by the development of a suitable theory of perception to complement a satisfactory post-Popperian philosophy of science. This theme becomes prominent in chapters on Kuhn and Feyerabend and a lengthy final chapter on perception. Popper's methodology—like Lakatos's—is completely rejected as an attempt to throw light on how scientific enquiry ought always to be conducted. Detailed argument for this rejection takes up about half the book, and Johansson's strategy is thorough: after some preliminaries, he lists over twenty rules, culled from Popper's methodological writings and assigned to homogeneous groups (*e.g.* rules demanding a high degree of falsifiability, acceptance rules for basic statements, rules for the social sciences), and then discusses them critically one by one. His reasons for rejecting them are various, but the most prominent ones concern difficulties about *ceteris paribus* clauses, auxiliary hypotheses and basic statements. A few rules have their own special objections. Johansson argues that neither Bernoulli's Theorem nor the requirement that falsification involve a reproducible effect constitutes a reason for Popper's rule for falsifying probability statements. Against methodological individualism he holds, *inter alia*, that there can be emergent properties of groups and non-reducible laws connecting such properties, and that, while a full explanation in terms of action involves a situational logic, the agent may be a group.

Johansson claims that a Popperian methodology is unsatisfactory and a new theory of perception is required. One of his main reasons for the former claim is that neither Popper's methodology nor Lakatos's variant of it can adequately deal with *ceteris paribus* clauses. He holds that *ceteris paribus* clauses are required for falsification and for falsifiability and, hence, for degrees of falsifiability. Let us take falsifiability first. He says: 'For a theory to prohibit any basic statement at all, a *ceteris paribus* clause is always needed' (p. 49; *cf.* p. 155). Now, on Popper's view (as I understand it), a falsifiable theory's potential falsifiers are those basic statements which, in the absence of special explanation, it prohibits. A theory's having a non-empty class of potential falsifiers does not presuppose that a *ceteris paribus* condition is actually satisfied. And different theories' having different associated *ceteris paribus* clauses does not prevent them having classes of potential falsifiers to be compared. (*Cf.* pp. 133, 155. Whether these classes are comparable and exactly what theories or systems of hypotheses are to be compared are further problems.) On falsification, Johansson claims that, if we are 'to get a falsification, it has to be assumed that the *ceteris paribus* clause used is more certain than the theory' (p. 39). But such an assumption is not required for the defence of Popperian restrictions on tinkering with *ceteris paribus* clauses; what is required is that it should be arguable that such tinkering prevents our learning from experience. Lakatos thought it more important to mention these clauses than Popper does because, also in the interest of learning from experience, he was against the quick kill and for the availability of ways to avoid it. I cannot see that Johansson has shown either theory of method to be deeply flawed because of an undue neglect of *ceteris paribus* clauses. These become important if falsificationism is rejected; their importance is not a reason for rejecting it.

Why should we aim to learn from experience? Taking the view that Popper is 'stepmotherly' (p. 119) in his treatment of basic statements and needs a theory of perception, Johansson argues that the hypothetical character of basic statements is different from that of theories. Of Popper's two main lines of argument for the hypothetical character of basic statements, one is that they entail laws, and his explanation of this point is unsatisfactory, as Johansson notes. The other is that experience does not constitute reasons for statements. This, according to Johansson, is convincing only as long as perceptual experience is thought of in terms of sense-data; his final chapter, which draws heavily on J. J. Gibson's *The Senses Considered as Perceptual Systems*, is devoted to criticism of sense-data theories and critical exposition of a new 'paradigm' which, he holds, would allow perceptual experiences to constitute reasons for basic statements and provide a defence of Popper's material condition on them (p. 190). This is an interesting contribution to discussion of the empirical basis problem although, even if correct, it leaves more to be said.

There is far more in Johansson's stimulating and closely argued book than can be adequately indicated here. However, his criticisms of Popper are sometimes misdirected. His supposition that Popper abandons the material requirement on basic statements (p. 49) is questionable (although it is not without textual warrant). And he wrongly attributes to Popper the view that basic statements should take precedence over all theories at all times (pp. 35, 43) and the view that they are more certain than theories (p. 192). More generally, he tends to see a belief, assumption or oversight where Popper is making a proposal (*e.g.* 'Popper seems to think that a rejection of a theory is, in contrast to an acceptance, always definitive', p. 74). And misunderstanding is twice betrayed by mistaken proposed amendments to texts of Popper's, one on page 86 and the other on pages 17 and 58 (where two distinct methodological rules are conflated). Feyerabend, too, is misread on page 145. But the scrupulous clarity of presentation makes it easy for the reader to see just where he disagrees with Johansson on questions of interpretation and of substance. And this is a virtue.

JAMES M. BROWN
New University of Ulster

RESCHER, N. [1976]: *Plausible Reasoning.* Assen/Amsterdam: Van Gorcum. Dfl. 29,50. Pp. ix+124.

Rescher's problem is an important and difficult one: what is the rational method for resolving inconsistencies in our belief system? A paradigm case occurs in 'whodunits': Who killed Cock Robin? There may be several reasonable alternatives—the owl, the sparrow, the wren, *etc.* Similar problems arise when scientists are faced with conflicting explanatory hypotheses or are trying to decide which part of a refuted system must be revised.

Rescher's theory of plausibilistic inference is intended to handle such cases of cognitive conflict. Many of his general remarks about the reasoning involved seem non-controversial if read using our ordinary, informal notions of plausibility, reliability, *etc.*: '. . . in cases of conflict, the more plausible should never give way to the less plausible . . .' (p. 55). It may happen 'that both P and ∼P should be relatively highly plausible' (p. 15); '. . . a thesis is more or less plausible depending on the reliability of the sources that vouch for it . . .' (p. 21). 'An inherently improbable claim can be "well attested", an inherently probable claim advanced "baselessly" ' (p. 38). But the more Rescher develops the details of his system, the less plausible it appears, to this reader at least. I will briefly describe his approach and indicate my most basic dissatisfactions with it.

Rescher's system works roughly as follows. We take incoming reports as axioms and assign each of them a preliminary plausibility index according to the relative reliability ranking of the source. We then extend our total body of propositions by drawing out certain logical consequences from the reports and this process may lead us to modify the original plausibility index assignments. Rescher construes 'information source' broadly so as to include eye-witness reports, expert testimony, tradition, memory, and even 'claim-authorising principles' such as simplicity or uniformity (p. 6).

So far, so good. But what is meant by the *reliability* of a source and how do we assess it? Rescher's answer is puzzling. He says that the question of reliability is quite different from the statistical question of how often the source proves to be correct. Rather we are to assess the 'solidity' and 'trustworthiness' of the source! But how is this to be done? Rescher admits that past performance enters in, but so does 'experience, informedness, training, *etc.*' (p. 7).

Here it appears to me that we *are* dealing with a statistical question after all, but are faced with the old problem of choosing the proper reference class. To assess Jones's reliability on a given occasion, we may need to look not only at Jones's past performances, but also at the record of similarly trained observers on similar occasions. Rescher is eager to show at every juncture that plausibility rankings are quite different from probability orderings, but I think he fails to show that probabilities cannot do the job or to present a detailed alternative account of reliability.

After we have ordered the sources according to reliability, they are given an index number (high numbers indicate high reliability). If a source of reliability *r* 'vouches for' a statement, it automatically is assigned *r* as its plausibility index. Rescher stresses that the plausibility assignment is independent both of the *content* of the statement and of its 'inherent probability'. Both claims are counter-intuitive. First, consider the relationship of plausibility and content. If a usually reliable source vouches for the statement 'all copies of Rescher's book have a pencil mark on page 24', I would certainly assign that claim less plausibility than the more modest assertion that my copy is so marked. Perhaps one could build such considerations into the original notion of source reliability and claim that we really are dealing with two sources here, but Rescher has not told us to do this.

Second, the relationship of plausibility and 'inherent probability'. Rescher may be falling into the common fallacy of ignoring prior probabilities described by Tversky and Kahneman [1974]. In evaluating the plausibility of the claim that the man sitting next to me is English, I should not only consider the strength of the observational evidence (bowler hat, umbrella, suede shoes), but also the prior probability of finding an Englishman there. Am I in Threadneedle Street or on a Hollywood stage set? (A complete analysis of the claim's plausibility would also require us to look at alternative explanations of the observational evidence.) Once again Rescher fails to supplant the standard probabilistic account.

Let us assume that we have now assigned the claims vouched for by each source a preliminary plausibility index. How, on Rescher's account, do these ever get modified? For example, suppose source A of reliability .8 vouches for P, but sources B, C, D, all of reliability .7, independently vouch for $\sim P$? On Rescher's account the plausibility of P stays at .8. Plausibility depends only on the testimony of the most reliable source; reports of less reliable sources provide neither confirmation nor disconfirmation. I would call this the Raskolnikov fallacy: I may be generally right to trust my own judgment, but not if too many lesser mortals disagree!

Once the axioms have been assigned indices according to the most reliable source which vouches for them, we begin to draw out logical consequences from them. According to Rescher, the conclusion of any argument is to be assigned a plausibility index not lower than the weakest link in the chain of reasoning which led to it.

Here plausibility clearly behaves differently from probability, for the plausibility of the conclusion $P \cdot Q$ will always come out to be as high as that of the least plausible conjunct. The question of whether what Kyberg calls 'conjunctivitis' is a disease or an important component of rationality has already been widely debated and Rescher's examples do not advance the discussion.

It is important to look carefully at any proposed counter examples to the multiplication rule for probabilities. If I tell the police the bank robber was wearing a green skirt *and* a red blouse, they may well consider my report on the suspect's total outfit to be (roughly) as plausible as my separate reports on skirt and blouse. But if they do so, it is in virtue of the fact that they do not consider the separate reports to be independent. They reckon that if conditions were such that I got the skirt colour correct, I am almost certain to have gotten the blouse colour right too. They would (or should) feel quite differently about my joint testimony concerning two different robbers on two different occasions.

Once again Rescher's argument that plausibility behaves differently from probability fails. But let us proceed with his theory. What do we do if the axiom set composed of reports from our sources is inconsistent? Considering that this was the problem Rescher began with, his answer is disappointingly vague. He considers it an advantage of his system that it can accommodate inconsistent premises. For example, he considers the following to be a 'perfectly viable plausibility-indexing': $/P/ = .5$, $/\sim P/ = .3$, $/Q/ = .2$ (p. 44). Of course, we must block as 'unreasonable' certain logical consequences from the set $\{P, \sim P, Q\}$, but Rescher gives us no guidance as to how to do this.

Neither does he advise us as to when the inconsistency of our axioms should lead us to revisions. If we do decide to revise, Rescher tells us to adopt a 'plausibility-tropic' strategy—that is, we should save the most highly plausible theses even if it involves keeping some statements of very low plausibility. Once again we see that highly reliable sources totally dominate the cognitive system. Again I would argue for a more democratic approach—somehow we should try to maximise content as well as plausibility.

Rescher then gives an analysis of induction and the problem of counterfactuals using his theory. The reader will probably find the plausibility of this treatment equal to that of the plausibilistic inference system itself.

NORETTA KOERTGE
Indiana University

REFERENCE

TVERSKY, A. and KAHNEMAN, D. [1974]: 'Judgment under uncertainty: heuristics and biases', *Science*, 185, pp. 1124–31.

BELL, J. L. and MACHOVER, M. [1977]: *A Course in Mathematical Logic*. Amsterdam: Elsevier/North-Holland. Dfl. 50.00 (U.S. $20.50). Pp. 600.

This book represents the contents of the M.Sc. course in mathematical logic and foundations of mathematics which the two authors teach at London University. It is a substantial work: nearly 600 pages put together with much loving care and close attention to detail. It presupposes some mathematical background, including elementary naive set theory. There are good exercises throughout.

It must have been a hard book to write. The authors have to find a place for everything basic in logic, they have to pack it all into one book's length, and they have to make it readable. I fear nobody has yet succeeded in this task. Shoenfield's *Mathematical Logic* was beautifully comprehensive but hardly readable. The present book is beautifully readable—the authors have gone to great lengths to make everything clear, and several of the chapters individually form excellent introductions to their subjects—but it has a number of gaps, and some parts are not up to date.

Chapter six is a model of how to begin recursion theory (with register machines). It also contains a complete account of Matijasevič's solution of Hilbert's tenth problem—the only textbook account of this important result. The authors swim against the tide by preferring formal arguments with the recursion theorem to informal ones with Church's thesis; but they may be right that this is pedagogically better, and the results are quite excellent. The chapters on intuitionist tableaux and non-standard analysis (9 and 11) present important things which are not easy to find elsewhere. The chapters on predicate logic (1–3), boolean algebras (4) and 'limitative results' (theorems of Church and Gödel *etc.*, chapter 7) are also good introductions, though the material is now standard.

Model theory (chapter 5) is less well served. This chapter is badly out of date, in a field where a number of important things have been happening. For example, there have been a steady stream of important applications in algebra, beginning with the 1965 paper of Ax and Kochen on Artin's conjecture. Incredibly, readers who want to find out about these applications are referred only to a book published in 1963. (Let us fill the gap at once, with G. E. Sacks, *Saturated model theory*, Benjamin 1972; and the Springer Lecture Notes in Mathematics volumes 454 by Hirschfeld and Wheeler, and 521 by Cherlin. Sacks's book will also plug some other gaps, such as quantifier elimination and stability.) Also the chapter is hampered by time-wasting boolean devices and by repetitions of things in earlier chapters.

The book makes no claim to be an encyclopaedia; but I think it is worth noting four important areas which are not mentioned, either in text or bibliographies. They are: consistency proofs for arithmetic, abstract definability theory, combinatorial principles, decision problems in algebra (apart from Hilbert's tenth).

It is clear, from remarks scattered throughout the book, that the authors care about foundations. Readers of this *journal* may well care about them too; so let me close with some comments on this. There is a fairly non-controversial kind of foundations, which consists in *describing* what methods or assumptions are needed for this proof, how that symbol can be informally defined, and so on. The authors do this sort of thing very well. (For two helpful examples, see p. 400 ff. on constructive meaning, and p. 572 f. on the nature of non-standard analysis.) But there are places in foundations, for example in choosing axioms for set theory, where one has to work harder and actually *justify* the assumptions or definitions. Here things begin to go wrong. It is not encouraging to be told at the outset that Platonism (here, the doctrine that sets exist independently of us) 'is in some ways a dangerous oversimplification and would certainly need—at least—major revisions if it were to become a component of a serious philosophy of mathematics', without being given any hint of what the dangers might be or what revisions would be needed. Worse is to follow: the unrestricted comprehension axiom is shown to be inconsistent, but at once 'because of its naturalness' some special cases of it are taken as axioms. Next, the axiom of regularity is not adopted until proved consistent, because 'it is by no means intuitively obvious'. But a footnote correctly points out that the axiom merely confines attention to those sets which are regular, and on this interpretation the axiom is extremely obvious (though the replacement axiom is less so). Fortunately none of this harms the mathematics, and I for one would rather praise the authors for raising these questions than blame them for giving silly answers. The fairest estimate is, sadly, that there is something rotten in the state of foundations.

WILFRID HODGES
Bedford College

BRITISH SOCIETY FOR THE PHILOSOPHY OF SCIENCE

Annual Conference

The Society's 1978 Conference will be held at the University of East Anglia, Norwich, on 22–24 September. The following sessions have been arranged:

Invention and Discovery in Mathematics—A Case History:
Professor G. Temple, FRS (Oxford)
Discussant: Dr J. Worrall (London)
Chairman: to be arranged

Probability, Information and Physics:
Dr P. M. Williams (Sussex)
Discussant: Mr J. Dorling (London)
Chairman: Dr D. H. Mellor (Cambridge)

Evidence and Explanation in Science:
Mr A. C. Ross (East Anglia)
Discussant: Mr. R. Bhaskar (Edinburgh)
Chairman: Professor G. R. Grice (East Anglia)

Explanation in Geography:
Dr B. A. Kennedy (Manchester)
Discussant: to be arranged
Chairman: to be arranged

The Conference is being organised on behalf of the Society by Mr D. S. E. Houghton of the School of Economics and Social Studies, University of East Anglia, Norwich NR4 7TJ, from whom further details may be obtained. (Please mark envelopes 'BSPS Conference'.)

AUSTRIAN WITTGENSTEIN SOCIETY

The Austrian Wittgenstein Society is pleased to announce the Third International Wittgenstein Symposium under the auspices of the Forum Philosophicum Austriacum, from 13 August to 19 August 1978 in Kirchberg/Wechsel, lower Austria (near Vienna). The theme of this year's symposium will be: "Wittgenstein, the Vienna Circle and Critical Rationalism", including a seminar on Popper's open society. The following sections are planned:

1. Wittgenstein's philosophy and its relation to the Vienna Circle
2. The Vienna Circle and its impact on the sciences
3. The impact of the Vienna Circle on contemporary philosophy
4. The foundations of society and of the social sciences
5. Popper's open society and the influence of his social philosophy.

Tentative participants include: Sir K. Popper (UK), K. Menger (US), H. Feigl (US), B. McGuinness (UK), W. Becker (BRD), C. Hempel (US), H. Mulder (Netherlands), W. Stegmüller (BRD), J. W. N. Watkins (UK). Scholars wishing to present a paper on a topic which falls within the above themes should request an abstract style sheet from H. L. Berghel, Department of Philosophy, University of Nebraska, Lincoln, NE 68588. Abstracts must be mailed no later than 31 May 1978 to either the President of the Austrian Wittgenstein Society, A. Hubner, A-2880 Kirchberg/Wechsel, Markt 234 Austria, or to W. Leinfellner, Department of Philosophy, University of Nebraska, Lincoln, NE 68588. Official languages will be English and German. Those planning to attend the conference should contact Dr Hubner at the above address for further information. Conference fees will be 350 Austrian Schillings (about 20 US dollars) for non-students and 100 AS (about $6) for students.

BOSTON UNIVERSITY
BOSTON COLLOQUIUM FOR THE PHILOSOPHY OF SCIENCE
1977–1978

Mario Bunge, McGill University, "The Nature of Life: Beyond Mechanism and Vitalism" (Commentators: Lynn Margulis, Boston University; Stephen J. Gould, Harvard University)	27 September 1977
Justin Leiber, M. I. T., "Talking with Extraterrestrials: Nominalism, Conceptualism, and Realism as Degrees in Radical Translation" (Commentator: Michael Martin, Boston University)	4 October 1977
Leszek Nowak, Editor, *Poznan Studies in the Philosophy of the Sciences* (Poland), "How to Overcome Marx: A Proposal of a Marxist" (Commentator: Thomas McCarthy, Boston University)	11 October 1977
Yves Barel, University of Social Sciences at Grenoble, "Mechanism and Structure: The Ambivalent War of Paradigms" (Commentator: Joseph Agassi, Boston University)	18 October 1977
Nelson Goodman, Harvard University, "Notes on a New Problem of Vision" (Commentator: Marx W. Wartofsky, Boston University)	25 October 1977
Richard M. Martin, Northwestern University and Boston University, "And" (Commentator: Judson Webb, Boston University)	8 November 1977
Edward MacKinnon, California State University at Hayward, "Atomic Physics: A Test for Theory Reconstruction" (Commentator: Leonard Nash, Harvard University)	15 November 1977
Edward Nell, New School for Social Research, "The Concept of Social Reproduction" (Commentator: Eric Chester, University of Massachusetts, Boston)	22 November 1977
Camille Limoges, University of Montreal, "The Locus of Power in Science" (Commentator: Lew Kowarski, Boston University)	29 November 1977
Humberto Maturana, University of Santiago, "Biology of Cognition: Life As a Cognitive Process" (Commentator: Jerome Lettvin, M.I.T.)	6 December 1977

Walter Feinberg, University of Illinois at Urbana-Champaign, "Intelligence, Intelligence Tests, and the Distribution of Knowledge" (Commentator: Ned Block, M.I.T.)	13 December 1977
Laurens Laudan, University of Pittsburgh, "The Poverty of Empiricism, Or, Philosophy Vindicated" (Commentator: Bruce Aune, University of Massachusetts, Amherst)	17 January 1978
Benjamin Rogers, Wichita State, "On Attributing Causes to Complex Individuals" (Commentator: Ronald Giere, Indiana University)	24 January 1978
E. V. Walter, Boston University, "The Places of Experience" (Commentator: Erazim Kohak, Boston University)	7 February 1978
A. I. Sabra, Harvard University, "Testing, Reform and Reaction: Three Endeavors of Arabic Astronomy"	14 February 1978
Robert Shaw, University of Connecticut, "Symmetries and Dualities in the Foundations of Ecological Science" (Commentator: Sheldon Krimsky, Tufts University)	21 February 1978
Peter Machamer, University of Pittsburgh, "Descartes and his Sixteenth Century Predecessors" (Commentator: Richard Popkin, Washington University)	28 February 1978
Philosophy of Technology: a Symposium Don Ihde, SUNY-Stony Brook, "Technology and the Transformation of Experience" Langdon Winner, M.I.T., "Technologies as Forms of Life"	7 March 1978
Howard Pattee, SUNY-Binghamton, "Dynamic and Linguistic Modes of Perception: A Cognitive Basis for Complementarity" (Commentator: Paula Menyuk, Boston University)	14 March 1978
Marsha Hanen, University of Calgary, "Evidence, Inference and the Law" (Commentator: Bernard Elevitch, Boston University)	28 March 1978
Daniel Bell, Harvard University, "The Question of Relativism in the Social Sciences" (Commentator: Alasdair MacIntyre, Boston University)	4 April 1978

LEV MITROKHIN, Institute of Philosophy, Academy of Sciences of the USSR, "The Marxist Concept of Religion: A Scientific and Philosophical Study" (Commentator: Robert S. Cohen, Boston University)	11 April 1978
JOHN J. STACHEL, Boston University and Institute for Advanced Study, "Science and Ideology: Some Themes for the Recent Marxist Discussion of Science and Ideology" (Commentator: Robert S. Cohen, Boston University)	18 April 1978
LOREN GRAHAM, Columbia University, "The World of Arthur Stanley Eddington; Science and Its Audience" (Commentator: John W. Yolton, York University)	25 April 1978
HERBERT MARCUSE, University of California, San Diego, "Esthetic Rationality" (Commentator: M. W. Wartofsky, Boston University)	2 May 1978

HISTORY OF SCIENCE

*is the **only** review of literature and research in the history of science, medicine and technology in its intellectual and social context.*

FOUNDED IN 1962 AS AN ANNUAL VOLUME, *History of Science* discusses problems and sources for future research in the history of science, medicine and technology, and surveys recent publications. It now appears each year in quarterly parts of about 80 pages each.

THE ANNUAL SUBSCRIPTION is £12.50 post free outside the Americas and Japan (£9.50 post free to direct private subscribers). New subscribers may purchase the first fifteen volumes (bound) for £62.50 post free, *including* the subscription to Volume 16 (1978). Write to: Circulation Manager, *History of Science*, Whipple Museum, Free School Lane, Cambridge CB2 3RH, England.

Philosophy and Phenomenological Research

AN INTERNATIONAL QUARTERLY

Edited by MARVIN FARBER

DECEMBER 1977, VOLUME XXXVIII, No. 2

CONTENTS

I. ARTICLES
  Distinctions Among Blame Concepts, by STEPHEN COHEN
  Two Problems of Being and Nonbeing in Sartre's *Being and Nothingness*, by RICHARD E. AQUILA
  The Bergsonian Period of Alfred Schutz, by HELMUT R. WAGNER
  On a Criticism of Marx on Law and Relations of Production, by W. SUCHTING
  On the Distinction between Perceptual and Ordinary Beliefs, by J. B. MAUND

II. DISCUSSIONS

III. REVIEWS, COMMUNICATIONS

IV. NOTES AND NEWS, RECENT PUBLICATIONS

Subscription prices $9.00 per year for individuals; $11.00 per year for institutions. Separate issues, $2.25 and $2.75, respectively. Postage and handling for back issues only. Papers submitted for publication will not be returned unless accompanied by a self-addressed stamped envelope, or equivalent postage.

A Circular listing the main contents of the Journal since 1940 will be sent upon request.

Address all communications to

PHILOSOPHY AND PHENOMENOLOGICAL RESEARCH

State University of New York at Buffalo, 320 Baldy Hall, Buffalo, New York, 14260

Synthese

An International Journal for Epistemology and Philosophy of Science

Editor-in-Chief: Jaakko Hintikka, Academy of Finland and Stanford University.

Associate Editors: Risto Hilpinen, Department of Philosophy, Harvard University,
Ilkka Niiniluoto, Institute of Philosophy, Helsinki.
Review Editor: Dagfinn Follesdal, Institute of Philosophy, University of Oslo.

Editorial Assistant: Esa Saarinen, Institute of Philosophy, Helsinki.

Board of Consulting Editors: David M. Armstrong, Yehoshua Bar-Hillel, Arthur W. Burks, Robert S. Cohen, Donald Davidson, Hans Freudenthal, Ian Hacking, Jerrold J. Katz, G. Nuchelmans, Wesley C. Salmon, Wolfgang Stegmüller, Erik Stenius, Patrick Suppes, Klemens Szaniawski, Ladislav Tondl, H. Tornebohm, Marx W. Wartofsky.

Synthese publishes articles in all the fields covered by the subtitle. These include the theory of knowledge; the general methodological problems of science; the methodological and foundational problems of the different departmental sciences; those aspects of symbolic logic and of the foundations of mathematics which are relevant to the philosophy and methodology of science, and those facets of the history and sociology of science which are important for contemporary topical pursuits.

Subscription price per volume of four issues Dfl. 110,– / US $44.00, plus Dfl. 15,– / US $6.00 for postage and handling. Two volumes will be published yearly. Personal subscription price on request.

D. Reidel Publishing Company
Dordrecht - Holland / Boston - U.S.A.

631

Erkenntnis

An International Journal of Analytical Philosophy

Editors: Carl G. Hempel, Princeton University, Princeton, N.J., U.S.A.; Wolfgang Stegmüller, Universität München, München, Germany; Wilhelm K. Essler, Universität München, München, Germany.

Erkenntnis is a philosophical journal for foundational studies and scientific methodology. Its contributions cover the following areas: that field of philosophy associated today with the notions of 'Philosophy of Science' and 'Analytic Phylosophy' (in a wide sense); the philosophy of language, of logic, and of mathematics; the foundational problems of physics and other natural sciences; the foundations of normative disciplines such as ethics, philosophy of law, and aesthetics; the methodology of the social sciences and the humanities; the history of scientific methods.

The decision to publish anew a journal under the name **Erkenntnis** calls for an explanation. It is not thought to make for a second time philosophical history with this name; nor will it be attempted to resurrect the philosophy of the Vienna and Berlin Circles – a well-nigh impossible task considering the 35 years that have elapsed since the old 'Erkenntnis' fell victim to the second world war; years in which the post-war philosophical discussion has in most respects progressed far beyond the positions of that time.

Building, though, both on the positive achievements and well-defined problems emerging from the critical discussion set in motion then, **Erkenntnis** stands well within the tradition of its predecessor with respect to its field of research. It also feels itself committed to that standard of conceptual clarity and rigor set by the old 'Erkenntnis'.

Subscription price per volume of 3 issues (500 pages) Dfl. 100,- / US $ 40.00 plus Dfl. 12,50 / US $ 5.00 for postage and handling. One volume will be published yearly. Personal subscription price on request. Copublished with Meiner Verlag, Hamburg.

D. Reidel Publishing Company
Dordrecht - Holland / Boston - U.S.A.

365

Noûs

Nihil philosophicum a nobis alienum putamus
Hector-Neri Castañeda, *editor*

Volume XI, Number 3 (September 1977)
SYMPOSIUM ON SPACE AND TIME
John Winnie, Guest Editor

John Winnie,*Introduction*; John Earman, *How to Talk About the Topology of Time*; Clark Glymour, *The Epistemology of Geometry*; Wesley Salmon, *The Philosophical Significance of the One-Way Speed of Light*; David Malament, *Causal Theories of Time and the Conventionality of Simultaneity*.

Volume XI, Number 4 (November 1977)

Geoffrey Hellman and Frank Wilson Thompson, *Physicalist Materialism*; Richard Cole, *Causes and Explanations*; Peter van Inwagen, *Ontological Arguments*; Alan P. Brinton, *Definite Descriptions and Context-Dependence*; Charles F. Kielkopf, *There is No Really Rigid Designation*; Nelson Pike, *If There Is No Necessary Being, Nothing Exists*; **Critical Reviews:** Ernest Sosa, *Gilbert Harman's* Thought; Mark Pastin *Keith Lehrer's* Knowledge.

Volume LXXXVII **MIND—APRIL 1978** Number 346
Edited by D. W. HAMLYN

Articles:
- The Law of Excluded Middle — N. COOPER
- Efforts to Explain all Existence — J. LESLIE
- Minimal Presuppositions and Maximal Metaphysics — J. W. N. WATKINS
- Metaphysical Possibility — L. REINHARDT
- Davidson on Learnable Languages — R. J. HAACK

Discussions:
- Why Probabilities Are Not Assigned to Propositions — A. A. DERKSEN
- Wittgenstein on Self-Knowledge — E. SANKOWSKI
- Borowski on the Relative Identity of Persons — R. PUCCETTI
- Comparing Space and Time Once More — G. SCHLESINGER
- Sortal Concepts and Identity — H. W. NOONAN
- Kenny and the Continuity of Wittgenstein's Philosophy — A. ELLIS

Book Reviews

Books Received

Notices

Published for the MIND ASSOCIATION by BASIL BLACKWELL
Orders to: Journals Department, Basil Blackwell & Mott Ltd.,
108 Cowley Road, Oxford OX4 1JF, England
Annual Subscription:
Institutions—£8.00 or $16.00
Individuals—£6.00 or $12.00
Single issues—£2.25 or $4.50

AMERICAN PHILOSOPHICAL QUARTERLY

PRIZE ESSAY FOR 1979

Topic:

THE CONSISTENCY OF BELIEF:
MERE DESIDERATION OR
ABSOLUTE REQUISITE?

Deadline:

JANUARY 15, 1979

Prize:

$100 and publication in the OCTOBER 1979 issue of the APQ

Conditions:

Only current subscribers to the APQ are eligible to make submissions. These must be written in English. They should be presented in the standard APQ typescript format and must not exceed 7,000 words in length. Entries will be judged by a panel of readers appointed by the editor of the APQ. The right of declining to make any award is reserved if no submissions of suitable quality are received.

AUSTRALASIAN JOURNAL OF PHILOSOPHY

EDITOR: BRIAN ELLIS

CONTENTS OF VOLUME 56, NO. 1, MAY 1978

ARTICLES

Incorrigibility and the Mental — **Gerald Doppelt**
Two Merits of the Circumstantial Operator Language for Conditional Logics — **I.L. Humberstone**
Ethical Universalism, Justice and Favouritism — **David Heyd**
Conclusive Reasons and Scepticism — **William S. Boardman**
The Argument from Psychological Egoism to Ethical Egoism — **Terrance C. McConnell**
Nonegalitarian Justice — **Derek Browne**

DISCUSSION

Belief in Dreams — **Charles E.M. Dunlop**
Sensations and Information: A Reply to Cornman — **George Pitcher**
Semantic Analysis in the Philosophy of Mind: A Reply to Ellis — **Alan Sussman**

CRITICAL NOTICES, REVIEWS, NOTES AND NEWS

ANNUAL SUBSCRIPTIONS: Individuals A$7.00 (US$9.00 or St. £5.00); Institutions A$10.00 (US$12.00 or St. £6.25) All correspondence should be addressed to the Editor or the Business Manager, Department of Philosophy, La Trobe University, Bundoora, Victoria, 3083, Australia.

dialectica

International Quarterly Review of Philosophy of Knowledge

Board of Directors Comite directeur Leitendes Komitee
Ed. Bertholet, case postale 68, 1880 Bex
F. Bonsack, 27, avenue de la Gare, Lausanne
J.-B. Grize, 15, La Traversière, 2013 Colombier
D. Jakubec, Devin 98, 1012 Lausanne
H. Lauener, Längfeldstrasse 8, 3063 Ittigen/Berne
J.-J. Loeffel, route de Sullens 8, 1030 Bussigny
B. Morel, rue Ecole-de-Médecine 16, 1205 Genève
P. E. Pilet, avenue Reymondin 13, 1009 Pully-Lausanne
W. Soerensen, «Les Chesaulx», 2036 Cormondrèche/Neuchâtel

Contents Sommaire Inhalt

Vol. 31 No. 3-4 1977

Paul Bernays
Vorwort
P. Bernays, Sprache und Wirklichkeit: Im Vorfeld der Fragestellungen
J. Hintikka, Language-Games
D. Davidson, Reality Without Reference
J. M. E. Moravcsik, Singular Terms, Belief and Reality
E. Kaeser, Physical Laws, Physical Entities, and Ontology
P. Janich, Die Sprache der Physik und die Wirklichkeit der Naturwissenschaften
F. Bonsack, Invariance as a criterion of reality
H. Lauener, Quine über Ontologie und substitutionelle Quantifikation
A. Graeser, On Language, Thought, and Reality in Ancient Greek Philosophy
P. Gochet, Model Theory and the Pragmatics of Indexicals
A. Stroll and R. Foelber, Talk about Talk about Surfaces
M. Black. More about metaphor

Errata
Received Books — Livres reçus — Eingegangene Bücher
Table annuelle vol. 31, 1977 — Contents of Vol. 31 — Inhalt Band 31

Subscriptions Abonnements Abonnemente	Switzerland Suisse Schweiz	Foreign Etranger Ausland
Subscription rate per annum (4 issues) Abonnement annuel (4 fascicules) Jahresabonnement (4 Hefte)	55.—Fr.s.	70.—Fr.s.
Price of single issues Prix du numéro Einzelnummer	14.—Fr.s.	18.—Fr.s.

Compte de chèques postaux/Postscheckkonto:
Dialectica Lausanne 10-5708
Bank Account/Compte bancaire/Bankkonto:
Swiss Credit Bank/Crédit Suisse/Schweiz. Kreditanstalt
Privatkonto 0112-11017-10

Distribution/Auslieferung
Dialectica, Case postale 1081, 2501 Bienne (Suisse)
Stechert-Hafner Inc., 31 East 10th Street, New York 3, N.Y.
B. H. Blackwell Ltd., Broad Street, Oxford, England

SCIENTIA

International Review of Scientific Synthesis - Founded in 1907

Scientific Editorial Board: Piero Caldirola · Ludovico Geymonat · Giuseppe Montalenti
Editor: N. Bonetti

SCIENTIA has never abandoned its programme of rigorous scientific synthesis; it aims at going beyond rather than refuting disciplinary specialization.

The motivations behind this approach can be explained today on the one hand through the formation of new areas of scientific research, and on the other through the lively evolution of those social frameworks which are at one and the same time both cause and effect of such research.

Scientia takes on the task it has set itself with the help of contributions from scientists and philosophers from all parts of the world.

Publication: 3 volumes a year of approx. 400 pages each. Articles are published in the language in which they where written and in the integral English and Italian versions.

Annual subscription: Italy Lit 18,000 - Europe Lit. 24,000 - Non-European countries $40.00
Specimen copy: Italy Lit. 2,000 - Europe Lit. 2,500 - Non-European countries $ 3.50

SCIENTIA - Via Guastalla 9 - 20122 Milano (Italy) - Telephone (02) 780.669

Journal of Philosophical Logic

Editor-in-Chief: Bas C. van Fraassen, University of Toronto, Ont., Canada.

Editorial Board: L. Aqvist, N. D. Belnap, Jr., M. J Cresswell, J. E. Fenstad, F. Fitch, D. Føllesdal, C. Hamblin, H. G. Herzberger, J. Hintikka, W. Hodges, S. Kanger, D. Kaplan, S. Kripke, K. Lambert, R. B Marcus, G. J. Massey, D. Prawitz, N. Rescher, W. C. Salmon, D. Scott, K. Segerberg, P. F. Strawson, P. Suppes, R. H. Thomason, G. H. von Wright.

Executive Committee: N. D. Belnap, Jr., B. C. van Fraassen, G. J. Massey, N. Rescher.

The journal is concerned with philosophical studies utilizing formal methods or dealing with topics in logical theory. Specifically included are: contributions to branches of logical theory directly related to philosophical concerns; contributions to philosophical discussions that utilize the machinery of formal logic; discussions of philosophical issues relating to logic and the logical structure of language; and philosophical work relating to special sciences (for example linguistics, history of logic, or physics), with emphasis on foundational problems.

Subscription price per volume of four issues Dfl. 100,- / US $ 40.00, plus Dfl. 15,- / US $ 6.00 for postage and handling. One volume will be published yearly. Personal subscription price on request. Reduced price for members of the Association for Symbolic Logic Dfl. 40,- / US $ 16.00 per volume, including postage and handling.

D. Reidel Publishing Company
Dordrecht - Holland / Boston - U.S.A.

THE BRITISH SOCIETY FOR THE PHILOSOPHY OF SCIENCE

Committee 1978-9

President: Mr L. J. Cohen
President Elect: Professor Gerd Buchdahl
Hon. Secretary: Mr. A. Young
Hon. Treasurer: Mr D. W. Miller
Members: Mr D. Hirschmann, Dr M. L. G. Redhead, Dr Y. Wilks, Dr P. M. Williams

EDITORIAL BOARD OF THE JOURNAL

R. B. Braithwaite
G. Buchdahl
L. J. Cohen
H. Dingle
Sir John Eccles
M. B. Hesse
Sir Harold Jeffreys
W. C. Kneale
S. Körner
J. L. Mackie
D. J. O'Connor
Sir Karl Popper
H. R. Post
J. W. N. Watkins
G. J. Whitrow
J. H. Woodger

The British Journal for the Philosophy of Science is the official organ of the Society, whose purpose is described by the following statement from its Constitution and Rules:

'The purpose of the Society is to study the logic, the method and the philosophy of science as well as those of the various special sciences, including the social sciences.'

The Society holds meetings, at which papers are read and discussed, once a month during the academic year. These are held, by kind permission, in University College, London. The Committee may recognise Regional Groups of Members formed to hold meetings in other parts of the country. Any Regional Group so recognised may nominate a representative Member to the Committee.

Ordinary membership is open to all interested persons on payment of the annual subscription. Membership of the Society is international and is by no means confined to people resident in the United Kingdom The annual subscription is £2.50 for U.K. residents and £1.25 for overseas residents ($2.50 if resident in U.S.A. or Canada). Those who wish to join should write to the Hon. Secretary, Mr Alistair Young, School of Social Sciences, University of Sussex, Falmer, Brighton BN1 9QH.

Brit. J. Phil. Sci. Vol. 29 No. 2 June 1978

The British Journal for the Philosophy of Science

Contents

Articles

117–130 PETER URBACH Is Any of Popper's Arguments Against Historicism Valid?

131–143 A. OLDING A Defence of Evolutionary Laws

145–177 YEHUDA FREUNDLICH Copenhagenism and Popperism

Discussions

179–183 DERYCK HORTON Accuracy of Prediction: A Note on David Miller's Problem

183–184 FRED D'AGOSTINO Sampson's 'Dilemma'

26 OCT 197

Review Article

185–188 SUSAN HAACK The Wheel and Beyond (review of N. Rescher: *Methodological Pragmatism*)

Reviews

189–208 JOHN POST on M. Fisk: *Nature and Necessity*; DAVID HIRSCHMANN on M. Grene: *The Understanding of Nature*; RYSZARD WÓJCICKI on R. H. Thomason (*ed.*): *Formal Philosophy, Selected Papers of Richard Montague*; JAMES BROWN on R. J. Ackermann: *The Philosophy of Karl Popper*, and I. Johansson: *A Critique of Karl Popper's Methodology*; NORETTA KOERTGE on N. Rescher: *Plausible Reasoning*; WILFRID HODGES on J. L. Bell and M. Machover: *A Course in Mathematical Logic*

B.S.P.S. Annual Conference

Announcements

CALCUTTA

Printed in Great Britain at the University Press, Aberdeen

Volume 29 Number 3 September 1978
BJPIA5 29 (No. 3) 213-310 (1978) ISSN 0007-0882

The British Journal for the Philosophy of Science

Published for the
British Society for the Philosophy of Science
by Aberdeen University Press

THE BRITISH JOURNAL FOR THE PHILOSOPHY OF SCIENCE

Editors: JOHN WATKINS and JOHN WORRALL

Subscriptions from January 1978
The *Journal* is published quarterly. The subscription price for mailing addresses in Britain is £7.00 net (U.S.A. and Canada U.S. $22.00, other overseas £11.00) per volume post free; single issues £2.50 net (U.S.A. and Canada U.S. $8.00, other overseas £4.00) plus postage. For members of the British Society for the Philosophy of Science—who should order direct from the publisher—the subscription price is £4.50 (U.S.A. and Canada U.S. $14.00, other overseas £7.00). Membership of the Society is international and is by no means confined to people resident in the United Kingdom. Orders may be sent to any bookseller or direct to Aberdeen University Press, Farmers Hall, Aberdeen, Scotland AB9 2XT.

Notes to Contributors
Articles, correspondence and books for review should be sent to John Worrall at the Department of Philosophy, London School of Economics, Houghton Street, Aldwych, London WC2A 2AE. Articles should be accompanied by abstracts of about 100 words.

Material is accepted for publication in the *Journal* on the condition that copyright is assigned to the British Society for the Philosophy of Science.

Three copies of all contributions should be sent, typed on one side only with wide margins and double-spaced. Footnotes, which should be as few as possible, should be typed separately, double-spaced at the end of the text. They should be referred to by consecutive numbering throughout the text, in which they will be inserted in page proof. Bibliographical references should be listed alphabetically at the end. The style of references should follow current *Journal* practice.

Long quotations are printed in small type without quotes, and should be so marked in the typescript. Single quotes should be used to form names of words and for shorter quotations; double quotes for other purposes. Diagrams, tables, and illustrations should be on separate sheets, with their desired position in the text clearly indicated. Symbolic formulae appearing as separate lines should be clearly distinguished in the text, and foreign letters (Greek, German, etc.) indicated in the margin. Material to be printed in italics, especially English letters used as symbols ('*A*', '*x*', etc.), should be underlined. On matters of style and layout, authors should consult CHAUNDY, T. W. *et al.* [1964]: *The Printing of Mathematics*. London: Oxford University Press.

Offprints of articles may be ordered when corrected galley proofs are returned. Fifty are supplied free; the price of extra copies is given on the order form.

© British Society for the Philosophy of Science 1978

For permission to reproduce material from *The British Journal for the Philosophy of Science*, please apply to the Editors.

Back Volumes
Volumes 1-16 may be obtained from Kraus Reprint Ltd., FL-9491 Nendeln, Liechtenstein or Kraus Reprint Corporation, 16 East 46 Street, New York, N.Y. 10017. Volumes 17-27 may be obtained from Aberdeen University Press, Farmers Hall, Aberdeen, Scotland AB9 2XT. Prices on application.

Brit. J. Phil. Sci. 29 (1978), 213–222 *Printed in Great Britain*

Three Myths of Computer Science

by JAMES H. MOOR

1 *Computer Programs*
2 *Software/Hardware*
3 *Digital/Analogue*
4 *Model/Theory*

In this paper I will discuss a basic concept of computer science—the concept of a computer program—and three related distinctions of computer science—software *vs.* hardware, digital *vs.* analogue, and model *vs.* theory. I believe all of these notions are important, and if properly understood, provide an important part of the conceptual framework of computer science. But, when these notions are misunderstood, certain myths tend to evolve. With regard to artificial intelligence work these myths perpetuate sloppy research, and with regard to philosophy these myths promote misconceptions related to the mind-body problem and scientific methodology.

1 COMPUTER PROGRAMS

Computers can be understood on two levels. Computers can be analysed on the physical level just as any physical system can. If the computer is electronic, then the laws of physics concerning electronics can explain the computer activity. But, it is the understanding on the symbolic level which makes computers calculating devices, for it is under this kind of interpretation that various structures or processes of the computer are understood as symbols. Similarly, a computer program can be understood on two levels. Physically, computer programs may be a series of punched cards, configurations on magnetic tape, or in any number of other forms. Symbolically, computer programs are understood as instructions to a computer.

Often, proposed definitions of 'computer program' are misleading. The following definition from a dictionary of computers is a typical example: A computer program is 'a set of instructions composed for solving a given problem by a computer' (Chandor [1970], p. 303). Initially, this definition seems reasonable enough, but a little reflection shows that it is both too narrow and too broad. First, consider computer programs which are

Received 28 *September* 1977

excluded by the definition. Computers can be and sometimes are programmed to produce other computer programs. The resulting programs are not necessarily *composed* for solving given problems by computer. In the simplest case the computer can generate random lists of computer instructions some of which turn out to be computer programs although the resulting programs were not designed for any given purpose. Furthermore, although computers do a lot of problem-solving, it is very misleading to characterise all computer activity in this way unless 'problem-solving' is taken in the broadest possible sense. Computers can perform all sorts of symbol manipulations. Sometimes computer programs do no more than instruct the computer to print certain characters. Secondly, consider items which are not generally considered to be computer programs but which are included by the definition above. If I stand in front of a computer and say, 'Calculate my tax return and report the results to the government', I would be giving a set of instructions composed for solving a given problem by computer, but in normal circumstances I would not be giving a computer program. Flow-charts often give sets of instructions composed for solving problems by computers, but not all such flowcharts are computer programs. One might be tempted to improve this definition by insisting that the instructions be at least be machine readable; but the problem with this suggestion is that often computer programs are printed on paper, scribbled on blackboards, expressed orally, or given in any number of other forms which are in general not machine readable.

I believe we can avoid these problems and still capture our intuitive notion of a computer program if we recast the definition as follows: a computer program is a set of instructions which a computer can follow (or at least there is an acknowledged effective procedure for putting them into a form which the computer can follow) to perform an activity. For instance, if for some instructions scribbled on a blackboard, one would have only to type them into a computer and the computer could follow them to perform an activity, then the instructions on the blackboard make up a computer program. Some flowcharts are computer programs but some are not. Those which are not may provide insight into the logic of an operation, but they are not given in a specific enough way so that an acknowledged effective procedure could put them into a form which a computer could use. Finally, the reason the oral instructions 'calculate my tax return and report the results to the government' fail to be a computer program is not because they are oral. Some computers today can execute a limited number of oral commands. Moreover, the instructions could always be typed into a computer. The problem is that no computer today could follow them. Of course, this is not to deny that there could be situations in the future in which such instructions could become a computer program, *e.g.*, a situation in which a computer is able to understand the instructions, has access to records of income, can communicate with the government tax office, *etc.* In principle virtually any physical entity or process could be understood symbolically as a computer instruction if one imagines an appropriate computer which could understand and execute the instruction. But as a practical matter what we regard as computer instructions, and consequently what we regard as computer programs, is determined by the computers available.

2 SOFTWARE/HARDWARE

Computer programs are often considered to be part of the software of a computer as opposed to the hardware. Unfortunately, computer hardware is frequently characterised as 'the physical units making up a computer system' (Chandor [1970], p. 179). By contrast this seems to suggest that software is not part of the computer system or even worse that it is not physical. It is important to remember that computer programs can be understood on the physical level as well as the symbolic level. The programming of early digital computers was commonly done by plugging in wires and throwing switches. Some analogue computers are still programmed in this way. The resulting programs are clearly as physical and as much a part of the computer system as any other part. Today digital machines usually store a program internally to speed up the execution of the program. A program in such a form is certainly physical and part of the computer system.

Furthermore, since programming can occur on many levels, it is useful to understand the software/hardware dichotomy as a pragmatic distinction. For a given person and computer system the software will be those programs which can be run on the computer system and which contain instructions the person can change, and the hardware will be that part of the computer system which is not software. At one extreme if at the factory a person who replaces circuits in the computer understands the activity as giving instructions, then for him a considerable portion of the computer may be software. For the systems programmer who programs the computer in machine language much of the circuitry will be hardware. For the average user who programs in an applications language, such as Fortran, Basic, or Algol, the machine language programs become hardware. For the person running an applications program an even larger portion of the computer is hardware. This pragmatic view of the software/hardware distinction makes the distinction both understandable and useful.

A myth concerning the software/hardware distinction can arise, however, if the distinction is understood and taken to have more ontological significance than it has. With regard to work being done in cognitive simulation, consider a distinction by Keith Gunderson between program-receptive features of mentality and program-resistant features of mentality ([1971], p. 70 f.). Gunderson says that program-receptive features include most kinds of problem-solving such as game-playing and theorem-proving; whereas, program-resistant features, Gunderson argues, are task-oriented and something clearly counts as success and failure in accomplishing the task. If a subject is problem-solving, he can provide protocol statements to report on how he is attempting to solve the problem and often the problem-solving abilities acquired by doing a few problems can be projected to new situations. Gunderson states that these properties generally do not apply to program-resistant features. The program-receptive *vs.* the program-resistant distinction seems plausible if Gunderson is simply suggesting that some mental abilities may be easier to program than others. But Gunderson makes it clear that he means much more than this. Gunderson ([1971], p. 73) says, 'It would be a methodological howler to attempt to simulate pains, feelings, and emotions with a machine simply by expanding current programming techniques in the sense of defining new routines. It may, of course, prove possible to produce machine analogues of these features through a development in hardware.' Gunderson ([1971], pp. 73–4) concludes, 'Thus, some psychological predicates may be viewed primarily as software predicates; some primarily as hardware predicates.' Consequently, Gunderson believes that those who do cognitive simulation work which attempts to program feelings, emotions, or various personality traits are making a basic methodological mistake involving the software/hardware distinction.

Gunderson's recommendations are generated by reading more into the software/hardware distinction than is there. What is considered hardware by one person can be regarded as software by another. Again, if at the factory the installation of certain circuits into a computer is interpreted on the symbolic level as giving instructions, then in that situation the resulting programs can be regarded as software.

Furthermore, even if one fixes the boundary between software and hardware at a particular level, *e.g.*, on the applications language level, there is still no reason to believe that it is a methodological mistake to attempt to simulate "program-resistant" features. For if one can simulate the features on the hardware of a digital machine, then this activity can be described in terms of a computable function. Such a function could be understood in terms of a Turing machine which can certainly be simulated through programming on the suggested software level.

Wherever the boundary between software and hardware is drawn, additional hardware is often helpful; for it is likely to increase software capabilities. In cognitive simulation there can be some very special, practical reasons for wanting more and somewhat unique hardware. For example, in simulating certain activities like perception some researchers believe it is desirable to have machines which have parallel processing capabilities to reduce the processing time and to make the programming somewhat easier. Other researchers have built robots with mechanical arms and television eyes. Watching robots controlled by the computer can make the simulation of certain cognitive activity much more realistic. But, the point is that capabilities should be cast into hardware only to the extent that there is no longer concern with changing them. For instance, to make a simulation of feelings more realistic one might build a piece of hardware which emitted an 'ow' sound when stimulated. We understand that an 'ow' sound should be omitted sometimes; but what we don't understand is what leads up to it and in general how the affective system is integrated with the cognitive system. It is investigation into these areas which requires software because we want to manipulate such capabilities in doing research. Thus, Gunderson is mistaken in claiming that programming is an inappropriate approach to simulating "program-resistant" features of mentality. On the contrary, in development of computer simulations of "program-resistant" features of mentality, programming is essential.

3 DIGITAL/ANALOGUE

Computers are often classified as one of two types—digital or analogue. There are also some hybrid computers which include both digital and analogue components. The essential difference between digital and analogue computers is generally taken to be in the type of information processing. In a digital computer information is represented by discrete elements and the computer progresses through a series of discrete states. In an analogue computer information is represented by continuous quantities and the computer processes information continuously. I believe this is a worthwhile distinction as long as it is understood that it is a distinction made on the symbolic level and not on the physical level of understanding the computer. Taken as strict physical descriptions the digital and analogue characterisations would be very misleading. Alan Turing puts the point nicely concerning discrete state machines.

> [Discrete state machines] are the machines which move by sudden jumps or clicks from one quite definite state to another. These states are sufficiently different for the possibility of confusion between them to be ignored. Strictly

> speaking there are no such machines. Everything really moves continuously. But there are many kinds of machines which can profitably be *thought of* as being discrete state machines. (Turing [1950], p. 439.)

There is also a discrepancy which accompanies the analogue characterisation if it is understood as a practical physical description. Theoretically, a dense continuum of values may be represented by a continuous physical quantity, but in practice the computer's sensitivity and a user's ability to discriminate these values are always limited. In practical terms the analogue computer seems capable of only a finite number of discrete states! A simple example is an ordinary slide rule, often regarded as an analogue computer, which is accurate only to a few places.

Such discrepancies are not important because the digital and analogue characterisations are not, or at least should not, be given as complete and accurate physical descriptions of the computer. Rather the digital or analogue characterisation is an interpretation on the symbolic level. The relevant physical feature will be abstracted. If it is a digital interpretation, continuities will be ignored; and if it is an analogue interpretation, discontinuities will be ignored. Undoubtedly, some physical systems are more easily interpreted in one way than the other. Nevertheless, in principle most, if not all, physical systems which might be considered to be computers could be interpreted either in digital or in analogue terms. For example, consider an early computer by Pascal which performed simple calculations by the movement of cogged wheels. Is Pascal's computer a digital or analogue machine? If we interpret the cogs in the gears as digits and understand the completed movements of the gears as discrete states, then Pascal's device is a digital computer. On the other hand, if we interpret the gears as representing a continuum of values and focus on the continuous movement, then Pascal's device is an analogue computer.

A myth about the digital/analogue distinction can arise if the distinction is given more ontological significance than it has, *i.e.*, if one believes that there are intrinsic physical properties which divide computers into one of these two classes. This myth can generate bogus questions such as 'Is the brain *really* a digital or an analogue device?' The brain can be interpreted in either way. If one treats neurons as being either in a firing or a non-firing state, then the brain can be understood as digital; and if one emphasises the ongoing chemical processes, then the brain can be understood as analogue. The "correct" interpretation will depend upon one's purposes.

Hubert Dreyfus ([1972], pp. 71–4) argues that researchers in cognitive simulation and artificial intelligence are misguided in their extensive use of digital machines because intelligent human behaviour stems from the brain which is most likely an analogue device. Thus, Dreyfus attempts to use the digital/analogue distinction to support his general conclusion that further significant progress in research in cognitive simulation or artificial intelligence is 'extremely unlikely' (Dreyfus [1972], p. 197). It is not sufficient in replying to Dreyfus to point out that digital computers can calculate whatever analogue computers can, for Dreyfus agrees that digital computers could simulate analogue computers at least with regard to the end result (Dreyfus [1972], p. 233). Dreyfus' concern is that digital computers will never be able to represent fully analogue information processing.

Dreyfus' argument is most forceful if one believes that the digital/analogue distinction ontologically separates computers into two classes, for this suggests that researchers in cognitive simulation and artificial intelligence are manipulating the wrong material. To use Dreyfus' analogy it is similar to the attempts of alchemists to turn lead into gold. Once it is realised that the digital/analogue distinction is not based upon intrinsic physical properties but is a matter of interpretation, then Dreyfus' argument loses much of its force.

Dreyfus might reformulate his view as follows: The conceptual scheme given by the analogue interpretation is more useful in characterising those information processes which lead to intelligent behaviour than that given by the digital interpretation. Now the issue becomes pragmatic. The claim might be put: Just as it is easier to solve certain differential equations on an analogue computer; it is easier to handle information processing leading to intelligent behaviour on an analogue computer. But, for Dreyfus' purposes, there are two clear shortcomings with such a position. First, the position as a criticism against cognitive simulation and artificial intelligence seems at best only hypothetical since so little is known about how knowledge, for instance, might be represented in a computer under any interpretation. Secondly, even if the claim could be established, it would not support Dreyfus' original, pessimistic conclusion that further significant progress in this area of research is extremely unlikely, but only that workers in cognitive simulation and artificial intelligence who use a digital analysis are labouring under a clumsy interpretation. Just as digital computers can be used to solve differential equations; it would seem that, however clumsily, processes leading to intelligent behaviour could be represented as accurately as one desired in a digital form.

4 MODEL/THEORY

A major activity in computer science is modelling. Computer models are constructed to simulate everything from the contraction of the heart muscle to the growth of rice crops. Of course, scientists have utilized models for centuries to help understand and test theories, and it is certainly appropriate for them to use the recently developed computer technology to continue to do this. However, computer scientists often speak as if there is no distinction among programs, models, and theories; and discussions slide easily from programs to models and from models to theories. For instance, Geoffrey Clarkson ([1963], p. 347) describes work on a model of the trust investment process as follows: 'Our model, written as a computer program, simulates the procedures used in choosing investment policies for particular accounts, in evaluating alternatives presented by the market, and in selecting the required portfolios'. A few lines later Clarkson says, 'Since our model is a theory of individual decision-making behaviour, the method of analysis is based on the theory of human problem-solving [by Newell, Shaw, and Simon].' There is nothing wrong with such talk as long as it is understood that is a *façon de parler* and it is realised that there are important distinctions among programs, models, and theories.

One can have a theory, *i.e.*, a set of laws used to explain and predict a set of events, without having a model except for the subject matter itself. Also, one can have a model of a given subject matter, *i.e.*, a set of objects or processes which have an isomorphism with some portion of the subject matter, without having a theory about the subject matter. For example, soap film on irregularly shaped wires is sometimes regarded as an analogue computer which gives solutions to minimisation problems (Isenberg [1976]). Although the soap film on the irregularly shaped wire can serve as a model, it does not generate a theory about minimisation. Today there is no mathematical theory which can provide a general existence proof for such solutions let alone an analytical approach for producing them. Such a mathematical theory might be developed some day, but of course the theory could be produced independently of models like the soap film model.

Further, a computer model is usually much more than just a computer program, for a computer program is a set of computer instructions in the sense defined earlier. For the computer model to be understood as a model the isomorphism has to be pointed out between the computer activity (as produced by the program, perhaps) and the subject matter in question. In general, one must give the computer activity a symbolic interpretation which goes beyond the standard symbolic understanding of the program in order to make it a model.

The model/theory myth occurs in computer science when the model/theory distinction is blurred so that programming a computer to generate a model of a given subject matter is taken as tantamount to producing a theory about the subject matter or at the very least an embodiment of a theory. As John Loehlin ([1968], p. 5) expresses it, 'A computer model of a system is a concrete embodiment of a theory about the operation of that system, and running it in a computer is a way of determining what the theory predicts under specified sets of conditions.' Such a claim is extremely misleading since a computer model does not automatically embody a theory in the important sense that one knows what the theory is. Understanding the isomorphism between the model and the subject matter does not necessarily reveal the theoretical connections which will allow explanations and predictions. The theory must be statable independently of the computer model. Only then can it be said in an interesting sense that the model embodies the theory, for only then can one view the subject matter or the model in terms of the theory.

This last point may seem unimportant since it might be thought that if somebody programs a computer to behave in a certain way, then he *must* have a theory about the subject matter which is being modeled. But, such a theory need not be at hand for the model may be produced by a number of *ad hoc* programming techniques. For instance, Joseph Weizenbaum [1965] has created a program, ELIZA, which by using scripts can direct the computer to carry on conversations. If ELIZA is given the psychiatrist script, the program is called "DOCTOR" and can guide the computer in interviewing a patient. When the computer is running ELIZA, it can be understood as a model of a language user holding a conversation. As Weizenbaum ([1976], pp. 6–7) points out, ELIZA has been successful in that some people become so emotionally involved that they converse with the computer as if it were a person. Weizenbaum himself wishes to argue that ELIZA does not contain a general solution to the problem of computer understanding of natural language. ELIZA works primarily by a somewhat superficial analysis of semantic and syntactic cues and certainly does not provide a theory of human conversation. Weizenbaum ([1965], p. 143) states, 'ELIZA in its use so far has had as one of its principal objectives the *concealment* of its lack of understanding'.

Patrick Winston states a common view about programs and theories. Winston ([1977], p. 258) says, 'Occasionally, after seeing what a program can do, someone will ask for a specification of the theory behind it. Often the correct response is that the program *is* [Winston's italics] the theory.' My claim is that this is rarely, if ever, the correct response. Even if there is some theory behind a model, it cannot be obtained by simply examining the computer program. The program will be a collection of instructions which are not true or false, but the theory will be a collection of statements which are true or false. Thus, the program must be interpreted in order to generate a theory. In the process of interpreting, it is likely that some of the program will be discarded as irrelevant since it will be devoted to the technicalities of making the program acceptable to the computer. Moreover, the remaining parts of the program must be organised in some coherent fashion with perhaps large blocks of the computer program taken to represent specific processes. Abstracting a theory from the program is not a simple matter for different groupings of the program can generate different theories. Therefore, to the extent that a program, understood as a model, embodies one theory it may well embody many theories.

I do not wish to minimise the importance of computer modelling, and I am not suggesting that the theory always must be fully worked out before the modelling can begin, for one of the purposes of constructing models is to work out a better theoretical understanding. But at some point the theory must be statable independently of the computer model. Otherwise gimmicky programming can pass for good research.

In conclusion, I would like to emphasise that I am not claiming that the distinctions which I have discussed are myths, for indeed the distinctions are very important to computer science. But when they are misunderstood, they can generate myths. Under such misunderstandings the hardware/software distinction and the digital/analogue distinction are made to do more work than they can do properly, and the model/theory distinction is not made to do enough work. When these distinctions are conceptually spindled, folded, or mutilated, they don't compute.

Dartmouth College

REFERENCES

CHANDOR, A., GRAHAM, J. and WILLIAMSON, R. [1970]: *A Dictionary of Computers*. Baltimore: Penguin Books.

CLARKSON, G. P. E. [1963]: 'A Model of the Trust Investment Process', in Feigenbaum, E. A. and Feldman, J. (*eds.*): *Computers and Thought*. New York: McGraw-Hill Book Company, pp. 347–71.

DREYFUS, H. L. [1972]: *What Computers Can't Do*. New York: Harper & Row.

GUNDERSON, K. [1971]: *Mentality and Machines*. Garden City, New York: Doubleday & Company, Inc.

ISENBERG, C. [1976]: 'The Soap Film: An Analogue Computer', *American Scientist*, **64**, pp. 514–18.

LOEHLIN, J. C. [1968]: *Computer Models of Personality*. New York: Random House, Inc.

MOOR, J. [1976]: 'Analysis of the Turing Test', *Philosophical Studies*, **30**, pp. 249–57.

TURING, A. [1950]: 'Computing Machinery and Intelligence', *Mind*, **59**, pp. 433–60.

WEIZENBAUM, J. [1965]: 'ELIZA—A Computer Program for the Study of Natural Language Communication Between Man and Machine', *Communications of the Association for Computing Machinery*, **9**, pp. 36–45.

WEIZENBAUM, J. [1976]: *Computer Power and Human Reason*. San Francisco: W. H. Freeman and Company.

WINSTON, P. H. [1977]: *Artificial Intelligence*. Reading, Massachusetts: Addison–Wesley. Publishing Company.

Brit. J. Phil. Sci. 29 (1978), 223–242 *Printed in Great Britain*

Testability of the Role of Natural Selection within Theories of Population Genetics and Evolution

by GERHARD D. WASSERMANN

1 *Dual Usage of the Concept 'Natural Selection'*
2 *Problems Posed by Genetic Variation and a Glimpse of Theories of Population Genetics*
3 *Views of neo-Darwinians and so-called 'non-Darwinians'*
4 *How Reliable is Artificial Selection?*
5 *Examples of Natural Selection*
6 *Are Selective Factors Essential in Population Genetics?*
7 *Is Fitness a good Diagnostic Indicator of Natural Selection?*
8 *Conclusions*

1 DUAL USAGE OF THE CONCEPT 'NATURAL SELECTION'

A perplexing issue in the philosophy of science concerns the extent and nature of testability of the *modus operandi* of natural selection in theories of evolution and theories of population genetics. I shall often refer the reader to Lewontin's [1974] thoughtful and philosophically written book, although I deviate from some of his opinions. Let me say at once that nobody disputes that natural selection occurs, since there exists an impressive list of well documented examples (see section 5). Yet whether natural selection works in the ways and on the scale that various theories have postulated, and whether any of these theories are testable, is quite another matter. From the start we must distinguish carefully between the usage of natural selection in two totally different types of explanations which try to throw light on how populations of organisms adapt to their environment. (1) The first kind of explanation is met within falsifiable equilibrium theories of population genetics, where several varieties of natural selection appear in the context of very general mechanisms (see sections 2 and 3). These theories try to explain in general terms how adapted states of sexually reproducing cross-breeding populations could be maintained approximately. They only descend to the level of particular species and their populations in order to test generally postulated processes. (2) The second kind of explanation, which relies on natural selection, is

Received 24 May 1977

found in attempts to 'explain' the genesis and maintenance of specific evolutionary adaptations by resort to existentially quantified *ad hoc* hypotheses, which sometimes may be linked hypothetically to theories of population genetic mechanisms. As neither these *ad hoc* hypotheses nor their specially postulated linkage with theories of general mechanisms are falsifiable, the hypotheses as well as the linkages are not testable. Such 'explanations' of adaptations are therefore metaphysical.

Failure to distinguish between the two listed types of explanations which invoke natural selection is liable to lead to unjustifiably generalised claims, such as Popper's ([1974], p. 120) conclusion that

> . . . the theory of natural selection is not a testable scientific theory, but a metaphysical research programme . . .

The term 'metaphysical research programme' is explicated in section 33 of Popper's [1974] paper. Such a 'programme' contains metaphysical ideas which are 'criticisable, though not testable'. To suggest that 'the theory of natural selection' contains metaphysical ideas can only make sense if one states very clearly what kind of theory of natural selection one has in mind, and this Popper did not do. I guess that Popper's [1974] text is directed against the second type of explanation discussed above, which relies on *ad hoc* hypotheses. Popper has provided no discussion of equilibrium population genetic theories and their usage of 'natural selection', and his failure to distinguish between the two usages of 'natural selection' could mislead various philosophers of science. Certainly his cited conclusion is fully and justly applicable to the usage of natural selection in connection with theories that invoke unfalsifiable *ad hoc* hypotheses.

Other thinkers were distinctly aware of the metaphysical abuses that occur when natural selection is combined with unfalsifiable *ad hoc* hypotheses to form a 'theory' of evolution or a part of such a theory. Waddington ([1967], p. 97) remarked at a symposium that

> Some physicists were saying that [the theory of evolution] is not a real theory at all; either it does not explain anything or it explains everything. . . . The criticism was that if an animal evolves one way biologists have a perfectly good explanation [based on unfalsifiable *ad hoc* hypotheses]; but if it evolves some other way, they have an equally good explanation [based on different unfalsifiable *ad hoc* hypotheses]. So what is the good of all this explanation? . . .

Lewontin [1972] supplied some, presumably fictitious, examples (which could be supplemented by many non-fictitious examples, but I do not wish to victimise particular culprits) of the metaphysical abuses inherent in the type of *ad hoc* theorising, about natural selection and evolution, to which the physicists mentioned would have objected.

> In particular a species of bird with a small bill may evolve a large bill size, because some aspect of the environment has changed so that large-billed birds now have more offspring. Or the species may split into two new contemporaneous species with different bill sizes, because geographically separated populations of the original species lived in different environments in one of which large-billed birds were more fecund, while in the other small-billed birds left more offspring. Such a theory can never be falsified for it asserts that some environmental difference [of a particular but unknown kind] created the condition for natural selection of a new character. It is existentially quantified so that failure to find the environmental factor proves nothing, except that one has not looked hard enough.

Lewontin asked pertinently

> Can one really imagine observations about nature that would disprove natural selection as a cause of different bill size? The theory of natural selection [as abused in this context, where it depends on hidden *ad hoc* hypotheses] is then revealed as metaphysical rather than scientific. Natural selection [when invoked in this *ad hoc* fashion] explains nothing because it explains everything.

We observe that Lewontin and Waddington's physicists have reached similar conclusions which refer to 'explanations' based on unfalsifiable *ad hoc* theorising. It seems pretty certain that this is also the type of theorising Popper had in mind. Undoubtedly *ad hoc* rationalisation is no use. Yet it sometimes forms a metaphysical cousin of a perfectly legitimate form of explanation. Let me illustrate by an example the marked difference between such a metaphysical and a typical scientific form of theorising.

Suppose that Smith contracts influenza. Then some metaphysical thinkers feel tempted to 'explain' Smith's infection by saying: there must have been someone from whom Smith caught the disease. But this is an existentially quantified and, hence, unfalsifiable metaphysical statement, which does not explain anything. If instead we ask What is the mechanism of influenza infection? then our answer may refer to a universally quantified scientifically testable theory involving the structure and mode of action of influenza viruses, their modes of propagation through the population and their pathogenic effects. Similarly if we ask What kinds of mechanisms are there in known testable population genetic theories? How do they involve natural selection? How have people tested these population genetic theories, including their postulated effects of natural selection? Could these theories explain general aspects common to many kinds of adaptations? then we are posing questions which refer to falsifiable, *i.e.* scientifically testable theories. This is not to say that the present falsifiable theories of population genetics are adequate. Once population geneticists have discovered more adequate testable theories, as they are trying to do, they can always probe further, by attempting to discover increasingly more specialised mechanisms (*e.g.* species-specific mechanisms and their testable theories which apply to populations of a species). Whereas such mechanistic explanations can proceed on a broad or narrow basis, they never proceed for equilibrium states in an *ad hoc* fashion. (By contrast nonequilibrium population genetic theories demand *ad hoc*, but sometimes plausible, initial conditions.) Although extant populations may require non-equilibrium population genetic theories (see below), equilibrium theories could provide crude first-approximation theories. Equilibrium theories do not try to answer unanswerable questions about historical origins (*cf.* the example of Lewontin cited above). As soon as historical questions are raised, sooner or later existentially quantified *ad hoc* hypotheses have apparently to be introduced to fill missing gaps. The only very general historical extrapolation which seems plausible (though unfalsifiable) is to suggest that if natural selection operates nowadays, then it is likely to have operated in the past.

We must avoid one possible error. Although some of the essential mechanisms of population genetic theories are testable in principle, they are at present not adequately testable in practice. This is not due to any metaphysical nature of these theories, but to limitations of available technical facilities. Some of the most important claims of prominent population genetic theories have therefore hitherto eluded sufficiently extensive testing (see section 2). Unfortunately these major claims are most intimately concerned with assessing the scale of the role played by various types of natural selection in extant theories. Much of population genetics is concerned with the statistical equilibrium distribution of alleles (*i.e.* variants of genes) at genetic loci in the gene pool of sexually reproducing cross-breeding populations. In population genetic theories the character states by which alleles are assessed may be chosen to be electrophoretic mobilities of protein types which are coded for by alleles at specific genetic loci. At some loci there may only exist a single type of allele for the whole population, and such loci are defined to be monomorphic. Two major types of theories have recently dominated population genetics, the first being 'balancing selection theories' (*cf.* Dobzhansky [1970], p. 128 for a typical example) and the second are what Lewontin [1974] termed 'neoclassical' theories. In section 3 I shall explain the difference of the two kinds of theories in some detail. Balancing selection is assumed to maintain approximate gene pool equilibria with specific, roughly fixed, frequency distributions of alleles at most genetic loci, the frequencies remaining fixed as the result of prevailing selective pressures.

Neoclassical theories assume an essential selective neutrality, *i.e.* selective indistinguishability, of most or all alleles at each one of those genetic loci where multiple alleles exist in the gene pool. To deepen our understanding of the technical meaning of the concept 'selectively neutral allele' it is useful to refer to a theoretical conclusion cited by Ohta ([1974], p. 351):

> If a mutant allele is selectively equivalent (neutral) to its wild-type allele, then the rate of mutant substitution in the population is equal to the mutation rate (Kimura [1969]).

In balancing selection theories (etc.) the postulated type of selection is introduced not *via* environmental 'selective factors' but by means of selection parameters. Regrettably, available techniques do not permit any sufficiently accurate measurements of these parameters (*cf.* Lewontin [1974], ch. 5), so that at best one can only guess their order of magnitude. Despite these uncertainties, the most prominent versions of balancing selection theories and of neoclassical theories have been falsified, partly semi-quantitatively and partly qualitatively (*cf.* Lewontin [1974], ch. 5 particularly pp. 267 ff.). Hence, according to Popper's demarcation criterion, the major population genetic equilibrium theories are falsifiable and therefore non-metaphysical. Granted that these somewhat juvenile theories are testable, their falsifications only suggest that we need better equilibrium theories, possibly hybrids between balancing theories and neoclassical theories. There exists also a pressing need to invent more extensively applicable testing methods (see section 2).

2 PROBLEMS POSED BY GENETIC VARIATION AND A GLIMPSE OF THEORIES OF POPULATION GENETICS

What are the central issues in population genetics? I suggested in section 1 that some of these issues are concerned with the search for mechanisms that can maintain equilibrium states of populations in which they are adapted to their environments. One way of assessing adaptation is to measure fitness. But this approach meets most serious obstacles in practice (see section 7). A more indirect approach relies on studying possible means for maintaining genetic variation in the gene pool, on the assumption that the latter leads to maintenance of the adapted state of the population. This raises what I shall call 'Lewontin's fundamental question'. Lewontin ([1974], p. 232) considered that 'The problem is What proportion of observed genetic variation is maintained by selection?' This, as we shall see (in section 3), is the central theoretical and empirical issue on which neo-Darwinians and so-called non-Darwinians differ. Genetic variation is achieved by formation of new types of alleles, including what non-Darwinians call wild-type genes, and the propagation of alleles through the gene pool of the population by means of recombinations. Formation of new types of alleles could result from mutational modifications of existing types, or through a combination of mutations and gene duplications (*cf.* Ohno [1970]) or possibly by other means (*cf.* Cook [1977] for novel possibilities).

At first sight the following claim by Popper might suggest that Lewontin's fundamental question is wrongly posed. Popper ([1974], p. 136) argued that

> Darwinism does not really *predict* the evolution of variety. It therefore cannot *explain* it. At best it can predict the evolution of variety under 'favourable conditions'. But it is hardly possible to describe in general terms what favourable conditions are—except that, in their presence, a variety of forms will emerge.

Here, again, as in the passage cited in section 1, Popper appears to have exclusively focused his attention on non-equilibrium theories which are based on *ad hoc* assumptions. His remarks do not apply to those testable population genetic equilibrium theories which attempt to explain the maintainance of variation, and resulting variety, in terms of universal mechanisms. Popper's allusion to 'favourable conditions' suggests that he is concerned with *ad hoc* existentially quantified circumstances. (The word 'Darwinism' which turns up in Popper's passage is ambiguous as it could refer either to neo-Darwinism or to a type of classical Darwinism.)

Any satisfactory answer to Lewontin's fundamental question must depend on the size of the fraction of the total number of genetic loci whose variation is individually phenotypically amenable to random sampling, either by available, or by as yet to be discovered, methods. The available molecular biological sampling procedure is (in higher organisms) restricted to random sampling of the protein products of a minute fraction of the total number of estimated genetic loci. The only widely useful tool for the sampling of genetic variation at individual genetic loci is gel electrophoresis applied to protein variants coded for by various alleles (if there are more than one) at each one of a very restricted set of genetic loci in the population. (It is likely that isoelectric focusing 'which separates proteins on the basis of isoelectric points' (*cf.* Powell [1975], pp. 107–11) may sometimes be used as an alternative to electrophoresis since it appears, in at least some cases, to be more sensitive than electrophoresis.) So far gel electrophoresis is only applicable to variants of soluble proteins, particularly enzymes which have been linked to an appropriate dye *via* enzyme-specific substrates which enable enzymes to bind that dye (*cf.* Hubby and Lewontin [1966]; Lewontin and Hubby [1966]; Harris [1966]; Prakash *et al.* [1969]; Lewontin [1974], pp. 99–109 and Powell [1975]). The method can as a rule only distinguish between those variants of a particular protein type which result from amino acid substitutions if these substitutions lead to differences in the total charge of the protein. Hence, gel electrophoresis is likely to underestimate substantially the number of alleles at genetic loci coding for enzymes. Isoelectric focusing promises a higher degree of resolution of protein variants at the same locus.

Higher organisms may contain 40,000–100,000 structural genes per haploid set of chromosomes, although these numbers are uncertain. These genes code probably for a number of protein types per individual that correspond, by order of magnitude, to the number of genes (unless there should exist, as in the case of immunoglobulins, many proteins with more than one polypeptide chain per protein). Yet, conservative estimates suggest that in higher organisms there may be only some 1,000 genetic loci coding for identifiable soluble protein types whose variants could be distinguished by contemporary electrophoretic techniques. Electrophoretic random sampling for soluble proteins extends in most species at present to some 5–40 distinct genetic loci (*cf.* Powell [1975]), *i.e.* in higher species to, say, 0·1 per cent of the genetic loci. Even if for some higher species, say 1,000, types of soluble proteins could be systematically sampled for this would probably only yield at most information about the amount of variation at 2 per cent of the genetic loci. The remaining 98 per cent of (at present) apparently insoluble or specifically unidentifiable protein types and their variants, of which many may be concerned with establishing and maintaining developmental patterns, in the face of natural selection (*cf.* Wassermann [1972], [1973]), cannot at present be sampled by electrophoresis or any other equally reliable and readily applicable method. But future molecular biological methods (*cf.* Powell [1975], p. 111) might be able to overcome these drawbacks and make most of the remaining 98 per cent of protein types available for random sampling of variation.

Even the humble sea urchin *Strongylocentrotus purpuratus* has more than 10,000 structural genes per haploid set of chromosomes (Galau *et al.* [1974, 1976]). This, and the situation noted above, has serious implications as becomes evident when we glance at Lewontin's [1974] passage which deals with a possible method for detecting balancing selection, and the doubts that could arise from the use of this method. He wrote:

> Suppose that 100 polymorphisms are subjected to a search for balancing selection and that the search is successful in 98. Then no reasonable person could doubt, if the polymorphisms were chosen without prior knowledge or bias about the forces of selection, that balancing selection is the chief cause of polymorphism. Suppose, however, that only two cases were proved. Then it might be that balancing selection is unimportant, but it also might be that the investigators did not try hard enough to find selective forces, or were not ingenious enough. That is why they want very much to find selection in the first place.

It appears that the gel electrophoretic method, despite its ingenuity, is insufficient at present, by a long way, for dealing in higher organisms with Lewontin's fundamental question, since optimally it only permits to detect potential protein polymorphisms at a few (say two) per cent of the genetic loci. It seems unsuited for detecting polymorphisms at any of the remaining, say, 98 per cent of genetic loci in the present state of the art. Likewise although amino acid sequencing, of a very small number of protein types for numerous species, suggests that during evolution some of the major sequences of essential proteins have become 'frozen in' (Ohno [1973]), possibly as a result of eliminative selection, we cannot conclude that this applies to most of the less conspicuous, say 99 per cent or more, protein types.

Harrison [1977] is certainly correct when he states that

> Electrophoretic surveys of levels of heterozygosity in animal species have established that populations often contain large amounts of genetic variation at loci coding for soluble proteins (Selander and Johnson [1973]; Powell [1975]; Selander [1976]).

Yet, exciting as these findings may be, they do not alter the fact that we are numerically amazingly close to the situation described above in Lewontin's fictitious example. We cannot adequately characterise, say, 98 per cent of the existing protein types of most higher species and their possible protein type polymorphisms. When it comes to testing for natural selection of protein polymorphs the situation is even worse. So far Harrison's [1977] extremely elegant method, based on environment-specific parallel variation at the locus coding for a particular enzyme type in sibling species of field crickets, has allowed to test for balancing selection in the wild at the locus of just one protein type (or possibly at a closely linked locus). Even if, as seems likely, his methods can be extended to other genetic loci and to other sibling species, then this approach could at the very best only assess the presence (or absence) of balancing selection at a very small percentage of genetic loci. Thus we would remain ignorant about the presence or absence of balancing selection at some 98 per cent or more of genetic loci.

Does this bleak picture simply mean that we may have to wait another few years, or another few centuries, until molecular biology has advanced far enough to use its already versatile methods in a sufficiently delicate manner to extend Harrison's type of approach, or related approaches, to most protein types of any population? Or does it mean, by any chance, that Popper's claim that natural selection theory represents a metaphysical research programme is generally valid? Does it mean that Popper's assertion extends from *ad hoc* theorising also to population genetics? The evidence shows that the research programme based so far on a combination of electrophoretic methods and the, apparently extendable, method of Harrison [1977] represent an empirical research programme directed towards discovering the extent of balancing selection in nature. These are early days in molecular population genetics. To proclaim that any extension of the present, extremely restricted, empirical research programme is impossible, Popper would have to be able to prophesy the future of molecular biological technology. I doubt whether many philosophers of science would be enthusiastic about such a forward projection.

The as yet underdeveloped state of empirical approaches to Lewontin's fundamental question seems to be paralleled by a growth of population genetic theories whose inadequacies call for improved versions. Although some extant theories are in principle and in practice testable, in practice the most systematic methods required for testing them appear to be precisely those required to answer Lewontin's fundamental question. Hence, not surprisingly, extant theories are no more empirically accessible than the present technological confinement of population genetics permits. Prevailing varieties of theories of population genetics have been formulated in two ways (1) as 'deterministic' theories (*e.g.* the balancing theories *cf.* Dobzhansky [1970], p. 128 and Watterson [1977]) and (2) as stochastic theories (*cf.* Kimura and Ohta [1971] and Ewens [1972] for neoclassical theories). These theories cannot be tested on an adequate sampling scale and their variables cannot be measured with sufficient accuracy (see Lewontin [1974], ch. 5; see also Nei [1975]). Moreover, (apart from a few recent exceptions) prevailing theories are equilibrium theories. The latter type of theories present enough obstacles as regards testing techniques, without our having to direct our efforts to the testing of idealised non-equilibrium theories. We cannot even be sure whether equilibrium theories are at least approximately adequate in many cases. Such theories rest on the assumption that although evolution can only be described by non-equilibrium theories, evolutionary change is so slow that adapted quasi-equilibrium states of populations often (but not invariably) may persist for long periods.

Next we must ask about the logical status of natural selection within theories. What sort of thing is natural selection, and what major types of natural selection have been classified? First, there is eliminative selection, which operates on the phenotypic expression of harmful or lethal alleles. Population genetic theories are, among other things, concerned with the statistical distribution of the genetic loci at which deleterious alleles occur, and with the influence of these alleles on temporary equilibrium states in the gene pool. Second, there is balancing selection, which I have already repeatedly referred to. Within population genetics any one type of natural selection may be regarded as a 'hypothetical construct' with precise theory-linked defining properties (*e.g.* selection parameters). Physics also uses profusely hypothetical constructs, such as 'fundamental particles' which only require meaning by their defining properties (*e.g.* charge, spin, strangeness and charm) within appropriate theories. Accordingly, the 'diagnostic' indicators of natural selection ought to be related to specific population genetic theories. This applies for instance, to Harrison's [1977] method of testing for balancing selection. Isolated examples of natural selection, which are not theory-related (see section 5), have no bearing on explanations of how natural selection operates in the wild in structuring populations. Moreover, what matters, as we saw, is an adequate sample size of genetic loci which can be tested for natural selection. Otherwise one cannot answer Lewontin's fundamental question. We must also ask whether so-called artificial (*i.e.* experimentally contrived) selection is relevant to any theory which tries to explain how natural selection is supposed to operate in the wild. I shall deal with this problem in section 4.

3 VIEWS OF NEO-DARWINIANS AND SO-CALLED 'NON-DARWINIANS'

Roughly, population geneticists can be divided into neo-Darwinians and so-called non-Darwinians. This contemporary division reflects present day types of theorising but might not apply to possible hybrid theories of the future. Many neo-Darwinians attach much importance to balancing selection and as a possible alternative they consider allelic-frequency-dependent selection. These alternative types of selection are the principle means which many contemporary neo-Darwinians envisage for fixing the frequencies of various alleles at most (multi-allelic) genetic loci within the gene pool of any sexually reproducing cross-fertilising population. Many neo-Darwinians attribute a small role to genetic drift, which is defined as a process that leads to random changes of allele frequencies at various genetic loci (Thoday [1975], p. 675). (Technically drift can be defined in terms of stochastic theories of population genetics.)

Many neo-Darwinians assume that selection fixes allele frequencies at most (multi-allelic) genetic loci, and that drift just imposes minor random variations at relatively few loci. The picture just presented is not that seen by Popper ([1974], p. 136), who thought that the neo-Darwinian theory of natural selection invokes

> various mechanisms by which not only variation but the whole hereditary material is controlled by elimination.

Thus, Popper disregarded the important role attributed by many neo-Darwinian population geneticists to balancing selection as a preserver of variation, and, instead, interpreted neo-Darwinism as if it were classical Darwinism (in the sense described by Lewontin ([1974], p. 195). To cite Dobzhansky ([1970], p. 97)

> Darwin and Wallace deduced natural selection from Malthus' principle that any population tends to increase in numbers in geometric progression, and consequently will sooner or later collide with the limited resources on which it subsists.

The deduced, postulated, 'classical Darwinian' natural selection led, in Darwin's opinion, to the elimination of many individuals of a population. That in addition to eliminative selection there could also exist balancing selection, which could maintain allelic frequencies in a gene pool, remained for Dobzhansky and others to demonstrate. At any rate, Popper's [1974] incorrect representation of current neo-Darwinian theorising is significant. It strongly suggests that whereas he condemned the 'theory of natural selection' as having a metaphysical character, he did not take into consideration that there exist nowadays various population genetic theories, in which several types of natural selection play different (and in principle testable) roles.

Balancing selection theories make the following assumptions. (1) Each individual of an appropriate population is heterozygous for a sizable proportion of its genetic loci. (On the basis of as yet very restricted electrophoretic random sampling, Powell ([1975], p. 81) concluded that 'on the average animals are heterozygous at about 28 per cent of their structural loci'.) This assumption remains to be more extensively tested by future technology. (2) For a majority of genetic loci of the gene pool there exist a considerable number of alleles. (3) Balancing selection fixes frequencies of alleles at most genetic loci of the gene pool, as already noted (*cf.* Lewontin [1974], p. 25). (4) There also exist harmful or lethal alleles, but many of these may be retained in the population via heterozygotes if there is heterozygotic overdominance (*cf.* Dobzhansky [1970], p. 128). Assumption (1) is known as the 'balancing hypothesis'. Assumptions (2) and (3) are not, by available technology, testable to the desirable extent; this is also true of assumption (1). In somewhat broader terms Lewontin ([1974], p. 196) noted that

> The balance school sees the maintenance of variation within populations and adaptive evolution as manifestations of the same selective forces, and therefore regards adaptive evolution as immanent in the population variation at all times. Because the alleles that are segregating in a population are maintained in equilibrium by natural [balancing] selection, they are alleles that will form the basis of adaptive phyletic change of speciation.

Let us now turn to so-called non-Darwinians (*cf.* Kimura and Crow [1964]; Kimura and Ohta [1971]; Ohta [1974]; Thoday [1975]; Ewens [1972]; and discussions by Lewontin [1974]). They embrace a population genetic view which Lewontin called 'neoclassical'. Non-Darwinians assign a major role to genetic drift, and make the following assumptions. (1) Although there may be many alleles at each one of numerous genetic loci of the gene pool, most of these alleles are selectively neutral, *i.e.* their phenotypic consequences are not selectively distinguishable. Hence, even if animals are heterozygous for many loci, heterozygosity (unless lethal or harmful) has no selective consequences at most heterozygous loci. (2) Because of the absence of differential selective pressures on most alleles their frequencies in the gene pool are variable and subject to substantial drift. (3) New types of alleles arise through prevailing mutation rates and fall into three classes. First there are selectively indistinguishable neutral alleles, second there are harmful or lethal alleles which form the bulk of selectable alleles and which are selectively removed from the population (*cf.* Lewontin [1974], p. 197), so as to preserve the *status quo* (Ohno [1973]), and third there occur at a very few loci rare favourable mutations and occasional rare alleles which participate in balancing selection (*cf.* Lewontin [1974], pp. 197–8). These assumptions show, incidentally, that Popper, in the quotation cited earlier in this section, has attributed to neo-Darwinians the mainly eliminative role of natural selection which characterises contemporary neoclassical non-Darwinian theorising.

The behaviour of a gene pool according to neoclassicism was described by Lewontin ([1974], p. 207) as follows:

> At any moment the majority of loci [in the gene pool] will [according to the neoclassical theory] be represented by only one allele, but decreasing proportions of loci will be represented by 2,3,4 . . . *n* alleles at varying frequencies. When this process has gone on for some time, a steady state will be reached [in the gene pool] as a kind of dynamic balance between the input of new mutations, the random increase of these mutations by drift, and the random loss of variation. We expect that the higher the mutation rate and the larger the population size, the more of this neutral variation will accumulate without being lost.

This raises the same unanswered empirical issues as neo-Darwinian balancing selection theories. Whereas one can see how neo-Darwinians can test for balancing selection in the wild (*cf.* Harrison [1977]) it is more difficult to see how one could reliably ascertain the supposed consequences of drift, for reasons that are discussed by Lewontin ([1974], pp. 208–10). Lewontin's arguments relate to the peculiar structures of the present prevailing types of neoclassical theories, and his arguments need not be repeated here. Yet, as Lewontin has appreciated, the neoclassical theory may be incomplete, and I suspect that a hybrid between neoclassical and a balancing selection theory may ultimately emerge. Unfortunately the available technology, discussed in section 2, seriously restricts at present the testing of any well constructed population genetic theory, to a degree below that which is desirable in order to answer Lewontin's fundamental question.

4 HOW RELIABLE IS ARTIFICIAL SELECTION?

Artificial selection is often considered as a means for testing population genetic theories (*cf.* Lewontin [1974], p. 250). But there remain, in my opinion, serious doubts about the role that artificial selection could or should play. Dobzhansky ([1970], p. 201) argued that

> Darwin used artificial selection as a model of the natural process; a mathematical theory of selection must almost necessarily be derived from experiments on artificial selection.

This belief, however (and its doubtful conclusion), rests on the implicit hypothesis that artificial selection necessarily simulates some natural selection process. One could only confirm this hypothesis by studying first the natural selection process extensively and then examine how well artificial selection simulates it. But if one could study the natural process in the first place, then one would not need any simulation, unless the simulation process could be better controlled (and provided the controls do not change the effects of the simulation process much from those of the natural process).

Dobzhansky ([1970, p. 104) may have felt uneasy about the implicit hypothesis just discussed. At any rate, he saw a need for justification and argued that

> Natural selection in the laboratory is not a contradiction of terms. When man chooses individuals to be preserved and bred and others to be eliminated or prevented from reproducing he practices artificial selection. If an experimental population in the laboratory is placed in an environment in which some genotypes have advantages over others, no discrimination by man towards producers and nonproducers [of offspring] is involved. Natural selection operates in the laboratory as well as in the wild.

Yet, if artificial selection should confirm a postulated form of natural selection (*e.g.* balancing selection) then this is no guarantee that this type of natural selection occurs abundantly in the wild. For all we know natural environments might not provide any selective factors which work like those that man imposes in the laboratory. It is for corresponding reasons that molecular biologists, after having shown that a particular mechanism functions *in vitro*, insist on showing that it operates also *in vivo*.

Artificial selection at best shows some of the types of selections that can occur, and the variety of character states that can be selected for in the laboratory. But this does not guarantee that any of these character states are ever being selected in the wild, where, for all we know, some or many of them might be selectively inefficient, owing to the absence of natural selective factors which correspond to the selectively effective laboratory factors. Accordingly the balancing selection theory, or other population genetic theories which involve natural selection, must be tested in the wild (*cf.* Harrison's [1977] method), if the tests are to be regarded as being relevant to the theory. It is therefore questionable whether a paper which reports a highly successful artificial selection experiment with bacteria *Pseudomonas aeroginosa* should be headed 'evolution in action' (Betz *et al.* [1974]), unless one could show that similar selective processes happen spontaneously in nature on a comparable scale. Related remarks apply to Rigby *et al*'s [1974] artificial selection of a strain of *Klebsiella aerogenesa* (despite the rather exciting gene duplication which apparently occurred in this case). Apart from this, the last two examples do not refer to universally quantified population genetic theories, but to particular instances of selection, of which I shall now give some more examples.

5 EXAMPLES OF NATURAL SELECTION

Some people believe that if they can exhibit a collection of examples of natural selection then this has significance (*cf.* Lewontin [1972] who thought so at that time, but not later). But examples of natural selection are only significant if they are related to a universally quantified population genetic (or more general evolutionary) theory (*e.g.* the work of Harrison [1977] which is relevant to the balancing selection theory). Otherwise such examples represent an ensemble of isolated case histories, which do not explain how selection functions or has functioned at the genetic level in such cases. Thus, the selection of plants by soils containing toxic metals (*cf.* Dobzhansky [1970], p. 218), the selection by DDT of DDT-resistant strains of insects (*cf.* Mayr [1963], p. 193) and industrial melanism in several species of moths (Hasebroek [1934]; Ford [1955]; Sheppard [1961]; Kettlewell [1956], [1961]) each demonstrate the existence of a particular instance of natural selection. But these instances *per se* are not necessarily relevant to population genetic theories. For example, the selective effect of toxic metals on plants has no obvious bearing on whether neo-Darwinian or non-Darwinian theories provide better accounts of population genetics.

6 ARE SELECTIVE FACTORS ESSENTIAL IN POPULATION GENETICS?

Some population geneticists and evolutionists feel tempted to formulate the 'diagnostic' indicators of natural selection as follows. They assert that natural selection demands that 'For every shift in the gene distribution within the population there exist appropriate environmental factors' (this formulation was presented to me by a philosopher of bioscience). But this diagnostic statement could be totally wrong for two major reasons. First, the statement refers to 'appropriate' but unspecified selective factors and is therefore existentially quantified and unfalsifiable. Moreover in the few cases where selective factors have been found these have been obtained by methods which are specific for each case. I conclude that the discovery of particular selective factors by a method specific for each factor is no more useful to a theoretician than the discovery of those isolated cases of natural selection which have no bearing on a population genetic theory.

To search for the effects of (unknown) selective factors on the gene pool, in relation to population genetic theories, is feasible (*cf.* Harrison [1977]). A critic might ask How do we know that a particular effect was due to selection unless we can actually show that there was a selective factor at work? We can silence the critic by noting that we do not have to find the infective carrier who infected Smith in order to demonstrate that Smith has influenza, we only have to show the effect, *e.g.* by isolating the influenza virus from Smith's serum. Similarly, if we can show convincingly that the frequencies of particular alleles (or of proteins coded by them) at a particular genetic locus have specific values in specific environments, and use sibling species as controls (Harrison [1977]), then we get at a theoretically significant effect, which suggests balancing selection at a particular locus, without knowing the environmental factor involved.

Let me now give a second argument which casts doubt on the need to demonstrate selective factors. The statement linking every gene shift with at least one environmental factor simply need not apply even if natural selection should be widely operative. For example, if a valid population genetic theory should be a hybrid between a balancing selection theory and a neoclassical theory, then the gene pool could contain many selectively balanced loci as well as many loci with selectively neutral alleles. If this were the case, then numerous shifts in gene distributions could be due to random shift, whereas others could be selectively conditioned. In this case the search for a selective environmental factor for every shift in the gene population would be futile, since the allelic shifts at many loci would be simply due to drift, notwithstanding the involvement of selection in maintaining allelic shifts at many other loci.

If one abandons the futile search for selective factors, then it is, I believe, methodologically sufficient to discover statistically significant correlations between particular character states associated with specific genetic loci and particular environments (*e.g.* as in the method of Harrison [1977]). If any one wishes to repudiate correlative approaches to population science on the grounds that they harbour the danger of spurious correlations, then he ought to repudiate on similar grounds most of the evidence correlating lung cancer with cigarette smoking and large numbers of other medical and scientific correlations. In order to gauge the extent of the occurrence of possible selection at a genetic locus, population geneticists have introduced selection parameters (*cf.* Dobzhansky [1970], p. 128 for selection parameters in a deterministic two-allele balancing selection theory). This, however, leads to difficulties in attempts to measure selection parameters accurately (*e.g.* *via* measurements of fitness, see below). The inability to obtain reliable measurements of selection parameters led to extensive attempts to guess their magnitudes (*cf.* Lewontin [1974], ch. 5). Nevertheless, such guesswork allows us to arrive at some qualitative conclusions which have proved sufficient to falsify one formulation of a balancing selection theory (*cf.* Lewontin [1974], p. 267).

7 IS FITNESS A GOOD DIAGNOSTIC INDICATOR OF NATURAL SELECTION?

Lewontin ([1974], p. 235) pointed out that

> The most direct approach to assessing selection is to measure the reproduction of the various genotypes at a locus and to calculate fitness values. . . . Although there is no difficulty in theory in estimating fitness, in practice the difficulties are virtually insuperable. *To the present moment no one has succeeded in measuring with any accuracy the net fitness of genotypes for any [genetic] locus in any species in any environment in nature.*

The reader is referred to Lewontin's detailed discussion of the obstacles which confront assessment of the fecundity and viability components in the wild. I must stress that although net fitness can be accepted as a theoretical measure for assessing the contribution of natural selection to adaptation, net fitness *per se* (as technically defined) does not fully describe or define the adapted state. Moreover the fact that people have up to now not succeeded in measuring net fitness in the wild with adequate accuracy excludes net fitness even as a mere assessor of the contribution of selection to adaptation. Accordingly one can in no sense equate fitness with adaptation either descriptively, in the sense of answering Lewontin's fundamental question, or (at present) as a yardstick for quantifying adaptation.

Those who claim that natural selection explains adaptation ought to distinguish between the attainment and maintainance of adapted states. Attainment, as mentioned, concerns historical questions and demands non-equilibrium theories, which may involve *ad hoc* assumptions and lead into the realm of metaphysical speculation. By contrast the question how different types of natural selection could assist in maintaining already attained adapted states may require only an equilibrium theory, and demands empirical answers to Lewontin's fundamental question. Attainment of adapted states could involve mutation pressure (*cf.* Ohta [1974]), possible non-random molecular biological processes (*cf.* Cook [1977] for some related speculative ideas) and selective effects. Once attained, these states could for long periods be maintained by a mixture of (1) extensive balancing selection at many loci and (2) random drift combined with eliminative selection at many other loci. Thus, neither attainment nor maintainance of adapted states need be expected to be exclusively explicable in terms of natural selection.

These conclusions differ from the assessment of the situation reached by Popper ([1974], p. 137), who wrote:

> Take 'adaptation'. At first sight natural selection appears to explain it, and in a way it does, but it is hardly a scientific way. To say that a species now living is adapted to its environment is, in fact, almost tautological. Indeed we use the terms 'adaptation' and 'selection' in such a way that we can say that, if a species were not adapted it would have been eliminated by natural selection. Similarly, if a species has been eliminated it must have been ill adapted to the conditions. Adaptation or fitness is *defined* by modern evolutionists as survival value, and can be measured by actual success in survival; there is hardly any possibility of testing a theory as feeble as this.

Popper did not distinguish between attainment and maintenance of adapted states. His passage seems to imply that either attainment or maintenance or both are fully explicable in terms of natural selection. Moreover, Popper appears to equate selection with eliminative selection, thereby disregarding balancing selection as a possible powerful instrument of maintenance. He did not seem to appreciate that population geneticists do not use the terms 'selection' and 'adaptation' in the simple tautological fashion described by him, which equates selection with eliminative selection. Instead population geneticists use selection in precise, technically defined, ways, so that they avoid tautologies of the type mentioned by Popper. In fact, if drift as well as balancing selection should play prominent roles in future improved population genetic theories, then eliminative selection alone could hardly be claimed to 'explain' (equilibrium) adaptation.

Fitness, as defined in certain population genetic theories, does not fully define any adapted equilibrium state of the gene pool. Moreover, fitness is not simply expressed by its viability component (what Popper termed 'actual success in survival'). It has also other components, notably a fecundity component (*cf.* Lewontin's [1974], pp. 238 ff. and p. 255 discussions). Prout ([1965], [1969], [1971]) and others, cited by Lewontin have shown the great importance of the fecundity component in evaluating partial fitness. Above all, in the passage cited above, Popper has not referred to a single contemporary population genetic theory. The type of non-equilibrium metaphysical theories which he seems to have had in mind, when he remarked that 'there is hardly any possibility of testing a theory as feeble as this', certainly exist in good measure, but must not be mistaken for testable population genetic equilibrium theories. The latter types of theories are universally quantified and do not try to explain by *ad hoc* hypotheses how innumerable specific types of adaptations could be attained and maintained with the help of various types of natural selection. The fact that numerous biologists have indulged in untestable *ad hoc* speculations about the manner in which specific adaptations could have been attained by natural selection does not vindicate their enterprise as being 'scientific'. Popper's attacks on such abuses of the concept of natural selection are fully justified.

8 CONCLUSIONS

The concept 'natural selection' continues to be used by biologists in two drastically different ways. (1) Evolutionists, and more specifically population geneticists, try to answer 'Lewontin's fundamental question', namely "What proportion of genetic variation is maintained by selection?". When used in this context 'natural selection' has technically defined properties which are related to universally quantified population genetic equilibrium theories. These theories are related to empirical research programmes. (2) Natural selection is also used in *ad hoc* ways to 'explain' adaptations in a metaphysical manner. Popper has rightly condemned this practice. Yet he failed to distinguish the two usages of 'natural selection' and thereby gave the misleading impression that any type of theory that involves natural selection forms part of a metaphysical research programme. Apart from this Popper has presented classical Darwinian doctrine under the inappropriate name of neo-Darwinism.

Attempts to answer Lewontin's fundamental question and various other issues of population genetics have raised numerous important methodological issues which I have briefly discussed. In particular I have examined critically the supposed need to search for 'selective factors' and the status of 'artificial selection'. I believe that the search for selective factors is redundant, and that artificial selection, although helpful in some respect is no substitute for answering how selection operates in the wild. In this respect the recent approach utilised by Harrison [1977] seems promising. It also appears that, in order to answer Lewontin's fundamental question, present empirical research programmes will have to be substantially, technologically extended.

The University of Newcastle-upon-Tyne

REFERENCES

Betz, L. L., Brown, P. R., Smyth, M. J. and Clarke, P. H. [1974]: 'Evolution in Action', *Nature*, **247**, 261.

Cook, N. D. [1977]: 'The Case for Reverse Translation', *Journal of Theoretical Biology*, **64**, 113.

Dobzhansky, Th. [1970]: *Genetics of the Evolutionary Process*. New York: Columbia University Press.

Ewens, W. J. [1972]: 'Sampling Theory of Selectively Neutral Alleles', *Theoretical Population Biology*, **3**, 87.

Ford, E. B. [1955]: 'Rapid Evolution and the Conditions which make it possible', *Cold Spring Harbor Symposia on Quantitative Biology*, **20**, 230.

Galau, G. A., Britten, R. J. and Davidson, E. H. [1974]: 'A Measurement of the Sequence Complexity of Polysomal Messenger RNA in Sea Urchin Embryos', *Cell*, **2**, 9.

Galau, G. A., Klein, W. H., Davis, M. M., Wold, B. J., Britten, R. J. and Davidson, E. H. [1976]: 'Structural Gene Sets Active in Embryos and Adult Tissues of the Sea Urchin', *Cell*, **7**, 487.

Harris, H. [1966]: 'Enzyme Polymorphisms in Man', *Proceedings of the Royal Society of London, Series B*, **164**, 298.

Harrison, R. G. [1977]: 'Parallel Variation at an Enzyme Locus in Sibling Species of Field Crickets', *Nature*, **266**, 168.

Hasebroek, K. [1934]: 'Industrie und Grossstadt als Ursache des neuzeitlichen vererblichen Melanismus der Schmetterlinge in England und Deutschland', *Zoologische Jahrbuecher*, Abteilung fuer Allgemeine Zoologie and Physiologie, **53**, 411.

Hubby, J. L. and Lewontin, R. C. [1966]: ' A molecular Approach to the Study of Genic Heterozygosity in natural Populations. I. The Number of Alleles at different Loci in *Drosophila Pseudoobscura*', *Genetics*, **54**, 577.

Kettlewell, H. B. D. [1956]: 'Further Selection Experiments on Industrial Melanism in the Lepidoptera', *Heredity*, **10**, 287.

Kettlewell, H. B. D. [1961]: 'The Phenomenon of Industrial Melanism in Lepidoptera', *Annual Revue of Entomology*, **6**, 245.

Kimura, M. [1969]: 'The Rate of Molecular Evolution Considered from the Standpoint of Population Genetics', *Proceedings of the National Academy of Sciences of the USA*, **63**, 1181.

Kimura, M. and Crow, J. F. [1964]: 'The Number of Alleles that can be maintained in a Finite Population', *Genetics*, **49**, 725.

Kimura, M. and Ohta, T. [1971]: *Theoretical Aspects of Population Genetics*. Princeton, N.J.: Princeton University Press.

Lewontin, R. C. [1972]: 'Testing the Theory of Natural Selection', *Nature*, **236**, 181.

Lewontin, R. C. [1974]: *The Genetic Basis of Evolutionary Change*. New York: Columbia University Press.

Lewontin, R. C. and Hubby, J. L. [1966]: 'A molecular Approach to the Study of Genic Heterozygosity in natural Populations. II. Amount of Variation and Degree of Heterozygosity in natural Populations *Drosophila Pseudoobscura*', *Genetics*, **54**, 595.

Mayr, E. [1963]: *Animal Species and Evolution*. Cambridge, Mass.: Harvard University Press.

NEI, M. [1975]: *Molecular Population Genetics.* Amsterdam: North-Holland.
OHNO, S. [1970]: *Evolution by Gene Duplication.* Berlin: Springer.
OHNO, S. [1975]: 'Ancient Linkage Groups and Frozen Accidents', *Nature,* **244,** 259.
OHTA, T. [1974]: 'Mutational Pressure as the Main Cause of Molecular Evolution and Polymorphism', *Nature,* **252,** 351.
POPPER, K. R. [1974]: 'Autobiography of Karl Popper' in P. A. Schilpp (*ed.*): *The Philosophy of Karl Popper,* vol. 1, pp. 1-181. La Salle, Ill.: Open Court.
POWELL, J. R. [1975]: 'Protein Variation in Natural Populations of Animals' in Dobzhansky, T., Hecht, M. K. and Steere, W. C. (*eds.*): *Evolutionary Biology,* **8,** 79. New York: Plenum.
PRAKASH, S., LEWONTIN, R. C. and HUBBY, L. J. [1969]: 'A molecular Approach to the Study of Genic Heterozygosity in natural Populations. IV. Patterns of Genic Variation in Central Marginal and Isolated Populations of *Drosophila Pseudoobscura*', *Genetics,* **61,** 841.
PROUT, T. [1965]: 'The Estimation of Fitnesses from Genotype Frequencies', *Evolution,* **19,** 546.
PROUT, T. [1969]: 'The Estimation of Fitnesses from Population Data', *Genetics,* **63,** 949.
PROUT, T. [1971]: 'The Relation between Fitness Components and Population Prediction in Drosophilia. I: The Estimation of Fitness Components', and 'The Relation between Fitness Components and Population Prediction in Drosophilia. II: Population Prediction', *Genetics,* **68,** 127 and 151.
RIGBY, P. W. J., BURLEIGH, B. D. and HARTLEY, B. S. [1974]: 'Gene Duplication in Experimental Enzyme Evolution', *Nature,* **151,** 200.
SELANDER, R. K. [1976]: 'Genic Variation in Natural Populations' in F. J. Ayala (*ed.*) *Molecular Evolution.* Sunderland, Mass.: Sinauer Associates.
SELANDER, R. K. and JOHNSON, W. E. [1973]: 'Genetic Variation among Vertebrate Species', *Annual Review of Ecology and Systematics,* **4,** 75.
SHEPPARD, P. M. [1961]: 'Some Contributions to Population Genetics Resulting from the Study of the Lepidoptera', *Advances in Genetics,* **10,** 165.
THODAY, J. M. [1975]: 'Non-Darwinian "Evolution" and Biological Progress', *Nature,* **255,** 675.
WADDINGTON, C. H. [1967]: 'General Discussion' (p. 97) in P. S. Moorhead and M. M. Kaplan (*eds.*): *Mathematical Challenges to the Neo-Darwinian Interpretation of Evolution.* Philadelphia: The Wistar Institute Press.
WASSERMANN, G. D. [1972]: *Molecular Control of Cell Differentiation and Morphogenesis.* New York: Dekker.
WASSERMANN, G. D. [1973]: 'Molecular Genetics and Developmental Biology', *Nature New Biology,* **245,** 163.
WATTERSON, G. A. [1977]: *Genetics* (in press).

Brit. J. Phil. Sci. 29 (1978), 243–263 *Printed in Great Britain*

Discussions

REPLY TO L. PEARCE WILLIAMS

One occasionally sees a response to a book review. It is inevitably by the author and he inevitably complains of being misunderstood and inevitably quotes himself endlessly in rebuttal. Very tiresome. One can have little sympathy for these exercises in self-justification.

Such used to be my attitude. But experience can sometimes wonderfully broaden the sympathies. In my case the experience was reading in this *Journal* (26, 241–53) a review of the first third of my book, *Fields of Force: The Development of a World View from Faraday to Einstein*. This review, by L. Pearce Williams, grossly misrepresents and then roundly condemns my book. Such a review puts the book's author into an awkward corner. If he does not respond he may appear to concede that there is no gross distortion of his book. If he does respond he may give the appearance of airing a purely personal grievance which, as such, is not really of any scholarly interest. Still, there is in fact an objective issue: the review may have unfairly harmed the chances of a book he regards as valuable. And he, as its author, is in the best position to reveal that the judgment of the review was based on a gross distortion of the book. Such considerations have led me to believe that a reply to Williams's review of my Faraday chapters would be the lesser of evils.

Let me focus on the main issue dividing Williams's and my views of Faraday, and leave more minor points to a supplementary note. The main issue is: what exactly was Faraday's world view from 1831 on, when he wrote his great *Experimental Researches in Electricity*? There are three connected problems here. The first is, what exactly was Faraday's fully developed world view? The second is, when did he complete its development? The third is, how did Faraday's and Boscovich's views differ?

On these issues I have three points to make. First, Williams does not even report my characterisation of Faraday's world view. Instead he astonishingly asserts that I am unaware of issues which are in fact the central concern of my account of Faraday's work. Second, Williams neither reports nor attempts to rebut my arguments for my views, and against his. Third, Williams's views are untenable and his arguments for them are inconsistent.

Williams says two things about my conception of Faraday's world view. The first is that I am ignorant of Faraday's belief that material particles are composed of lines of force. He says (p. 255): 'Berkson can write "when many particles are strung along a line of force" without realising that the strained particles *are* the line of force.' Though the quoted phrase does not positively indicate that the particles are composed of lines of force, *on the same page* (p. 86) as the quotation Williams gives I say: '... the particles themselves are converging lines of force.' Indeed, Faraday's field theory of matter is one of the main themes of my book. The page Williams quotes from begins a section explaining that theory. The influence of this theory on Faraday's work is discussed throughout the first third of the book, and is returned to for each major figure: Maxwell, Lorentz and Einstein.

The second comment on my conception of Faraday's world view is the following: '. . . quite rightly [Berkson] single[s] out Faraday's idea of the field, of the ability of space itself to be the vehicle of force, as his most daring and important conception' (p. 250). In fact, I take care to separate myself from the view Williams ascribes to me. I address this issue for example on my page 318: 'Does Faraday *identify* the field with space, as Einstein does in his generalized theory? In one sense we may say that Faraday did not identify the field and space, and so differs from Einstein. That is, Faraday never dreamt of curved space and of the properties of the field as part of the geometry of this space. But in another sense Faraday may well have thought of space and the field as the same. This is that the abstract space of Newton might have no effect on the working field. . . . We have no clear evidence on this issue, however.'

Williams never reports my actual characterisation of Faraday's fundamental ideas. Let me summarise my ideas briefly, to make clear how far Williams is from discussing the actual contents of my book. Two ideas, I believe, were fundamental to Faraday's metaphysics. The first was the idea that *force is the sole physical substance*. By this I mean that Faraday assigned to force the traditional properties of physical substances: having a unique location in space, and having essential properties conserved through continuous changes in place and time. This idea of force as a substance resulted in a number of Faraday's characteristic beliefs: the belief that there is only one kind of force (electric, magnetic, and gravitational force are not essentially different); the belief that there is 'conservation of force'; and the belief that there is no essential distinction between force and matter (the key element of his unified field theory). These beliefs of Faraday, as I explain in detail, decisively influenced his experimental work.

The second fundamental idea of Faraday's metaphysics is well known. He rejected instantaneous actions at a distance. Faraday's substitute for the Newtonian idea of action was his notion of 'contiguous action', the idea that each bit of matter/force acts only upon its immediate neighbours. When we combine these two fundamental ideas, we get Faraday's vision of the physical world: a continuous sea of force-substance, each point of which interacts with its neighbours only.

This conception of Faraday's world view gives the view a place distinct from both the 'ether' theorists and that of Einstein. Faraday's difference from Einstein is that he did not reject the notion of physical substance as Einstein did. Einstein's idea that multiple masses and shapes, none of them preferred, could be assigned to objects was foreign to Faraday. For Faraday the notion of substance was fundamental.

However, Faraday's view was also distinct from that of the ether theorists. He explicitly rejected the ether. His fundamental disagreement with the ether theorists, I believe, was that he did not believe that the laws of *Newtonian* mechanics governed the interaction of a force point with its neighbours. All the pre-Lorentz ethers adopted Newton's mechanics as a matter of course. Thus we have Faraday with a position independent of both ether theory and Einstein. This conception of Faraday's metaphysics, I should note, also enables us to identify the cause of the vagueness (acknowledged by all) which appears at key points of Faraday's theories: He was unable to produce any of the general laws which should govern the basic process of interaction between neighbouring forces.

So far I have explained that Williams misreports some of my views on Faraday, and ignores my main contentions about Faraday. On one issue he gets me right, but another kind of distortion comes in. Williams rightly notes (p. 251) that I regard 1831 as the crucial date for the creation of Faraday's field theory. And he correctly reports my belief that in 1831 Faraday's views were fundamentally different from Boscovich's. Williams dismisses my opinion out of hand as an "elementary" error, due to my being "impatient", and says that as a result of my elementary error I 'make a hash out of [Faraday's] scientific endeavours' (p. 250). In addition, Williams reports none of the arguments I give for my views and against his.

Williams thus gives the impression that my views have been adopted most thoughtlessly, without a critical examination of their relation either to his own views or to the evidence. And here is the distortion: whether or not my views are correct, my book in fact contains a critical examination both of the alternative views and of the relation of the evidence to those views. In particular I specifically note (p. 331) that my main disagreement with Williams's *Michael Faraday* is that it fails to note Faraday's divergence from Boscovich's views. I give a series of arguments for the thesis that Faraday's views were different in important ways from Boscovich's (see, for example, my pp. 50–1 and 117). Further I explain throughout my Faraday chapters how the distinctive views of Faraday were important for Faraday's experimental work.

Instead of reporting and attempting to rebut my arguments, Williams gives an argument for the virtual identity of Faraday's and Boscovich's views, or at least for their identity before 1850. To show that the identity of the two views is not such an 'elementary' truth (I think it is quite false), let me examine Williams's argument.

Williams says of Faraday that 'All that he got from Boscovich was the idea of point-atoms, surrounded by forces. How these forces existed did not trouble Faraday in his early years. What seems ever to have been before his eyes was these articles and their associated forces' (p. 251). Later, on the same page, he emphasises the Boscovichian character of the views expressed by Faraday 1844: 'The atoms or particles Faraday discusses are clearly Boscovichian . . . Boscovich's atoms [like Faraday's] are of infinite size as reference to his famous curve will show for there the attractive force of gravity specifically extends to infinity.'

Williams cannot have it both ways. Boscovich cannot advocate point-atoms and atoms infinite in size. My point here is not a quibble; Williams's inconsistency is indicative of the confusion which results if we identify Faraday's and Boscovich's views. Understanding the distinction is essential to understanding Faraday. Boscovich's atoms are *point masses* in empty space which exert forces instantaneously at a distance. But the material particle cannot be identified with the force diagram. The particle is the point mass, not the pattern of forces; *force* and *matter* are different for Boscovich. The two are the same for Faraday. The identification of force and matter is the foundation of Faraday's field theory of material particles (which I am supposed to be ignorant of, see above).

In fact, a number of basic features of Faraday's thinking were incompatible with Boscovich, and this is why Williams's claim of the closeness of the two views (before 1850) is untenable. First, the denial of action at a distance for electricity and magnetism is incompatible with Boscovich. When an atom of Boscovich moves, the whole force pattern must move with it instantaneously. Already in his sealed note to the Royal Society in 1832—where he claims priority for rejecting action at a distance for electricity and magnetism—Faraday speaks of magnetism in a wave from one magnetic body to another. Similarly, Faraday's notion of contiguous action, which at the latest dates from 1837 (where Williams puts it), is incompatible with Boscovich. Boscovich's atoms act directly on all other atoms in the universe, and not merely on contiguous parts of the field, as in Faraday. Thus the differences between Faraday and Boscovich are essential to understanding the development of Faraday's scientific work in almost the whole of the *Experimental Researches*. And, contrary to Williams (p. 252) Faraday had many reasons to reflect on the action of one body on another at a distance.

Williams only concedes in a footnote to his book that after 1850 Faraday 'subtly changed' Boscovich. He says (*Michael Faraday*, p. 464), 'Faraday's concept of gravitation as a strain also subtly changed his picture of the Boscovichean atom. To Boscovich, the forces of his atoms were forces acting at a distance; by denying action at a distance, Faraday reduced the Boscovichean atoms to centres of strain.' But the point is, Faraday already rejected action at a distance near the beginning of his *Experimental Researches*, twenty years earlier. Williams view of the matter is, in fact, a non-starter.

Note

Here I note Williams's distortions of my book—and his own!—on secondary issues.

(1) Williams mistakenly identifies my views and those of Agassi, and condemns us together. In the first paragraph of his review he implies that Agassi and I agree both on what is Faraday's world view, and on the development of field theory after Faraday. In fact on both we are in fundamental disagreement (though I am an admirer of Agassi's—and Williams's—book).

Again, Williams ignores my book; I have a detailed discussion of my disagreements with Agassi (pp. 332–4). My basic disagreement with Agassi's view is that he wrongly identifies Faraday's and Einstein's world views. He believes that Faraday's innovation was to conceive of force as 'property without a substance'. Agassi's view does not enable us to explain a number of features of Faraday's world view. And, as I argue, it is not called for by Faraday's problem situation, as Einstein's denial of substance was called for by his. Williams also has three further mistaken ascriptions of Agassi's views to me: First, I did not write my book to 'illustrate a philosophy', as Agassi did. Second I do not endorse (or reject) Agassi's psychoanalytic interpretation of Faraday's ailments. Third, I do not say, as Williams alleges (p. 251), that Faraday identified electric current and light.

(2) Another issue is whether Faraday met with opposition from the scientific community of his day. The curious fact is that Williams attacks me on points where his book and mine are in agreement. Throughout his book, Williams

speaks of and documents opposition to Faraday's theories. For example Williams says (p. 78): 'The fact that both Davy and Faraday knew that their atomic theories would be dismissed as metaphysics at best or meaningless at worst led them both to keep them in the background.' And in my interpretation of Stokes's criticism of Faraday's electrogravity paper, I am following Williams. Yet Williams attacks me on this point. In his review he asks rhetorically (p. 249), 'Was it really, as Berkson writes, "pressures from the establishment in the scientific community" that caused him to withdraw his paper, or was it, merely, that he recognised the good sense in Stokes's remark that purely negative results had little to offer.' In the book Williams says Stokes's criticism was 'based upon a rejection of Faraday's whole vision of the unity of force', and that such a view does 'throw considerable light upon the relation of Faraday's thoughts to those of his more orthodox colleagues' (p. 470).

Incidentally, Williams, pages 247–8, misreads me to say that Faraday felt a constant burden of struggle to be understood. What I say is that Faraday felt a constant burden of struggle *for* understanding, understanding of nature. (See my pp. 17–18.) Another misreading is of my critique of Faraday's account of unipolar induction. Unaware that I am criticising and not reporting, he says that I am ignorant of Faraday's opinion that the lines of force are resting during unipolar induction. In fact, on the very page that Williams cites (p. 107) I state, '. . . [Faraday] thought of the lines of force as resting when the magnet rotated.'

(3) A third point is whether Faraday believed in 1831 that the lines of force have independent physical existence. In attacking me Williams again contradicts his own book. He says against my view that the 'insights that make up the first volume of the *Experimental Researches* simply make no sense within the framework of "lines of force . . . considered as an independent physical existence"' (p. 252). But in his own book Williams agrees: 'The independence of the lines of force introduced a new element into Faraday's researches. . . . It is no exaggeration to say that a fundamentally new way of looking at physical reality was introduced into science in this Second Series of the *Experimental Researches* [done in 1831].'

On a related point, Williams says that there are 'masses of evidence' (p. 251) against my view that Faraday had his field theory—especially the rejection of action at a distance—before his work on electrochemistry. Williams cites none of the supposed masses of evidence, nor does he report the evidence I give for my views. Two pieces of evidence I give are the sealed note of 1832, written before the experiments on electrochemistry, and the adumbration of his later views on electrochemistry in his first paper on electromagnetic induction (*Experimental Researches*, art. 76).

(4) A final issue is the errors in transcription pointed out by Williams. The errors are real enough, and I regret them; but Williams attaches an importance to these errors which strikes me as highly exaggerated. My mistake in one of the transcriptions (see Williams, p. 244) is a missing set of brackets around an explanatory phrase, and some missing dots. The result is that the phrase 'common current' instead of 'common electricity' is ascribed to Faraday, with no change in the meaning of the quotation. The quotation denies an essential distinction between electricity from static (common) and voltaic sources. As the meaning is not violated, and as the *Experimental Researches in Electricity* is one

of the most widely reprinted books in the history of science, I think Williams's claim that I have done 'mortal injury to the historical record' (p. 244) is nonsense.

The other erroneous transcription Williams points out is my long quotation from Faraday's (published) *Diary*. I failed to italicise any of the words italicised in the original, and I reversed the order of some words, fortunately not altering the meaning. These errors Williams classes as 'venal' sins. (Incidentally, my omission of Faraday's cross-reference numbers is not an error, but standard practice followed, *e.g.* by Williams.) What I gather is supposed to be "positively scandalous" is the fact that in one sentence the word 'by' is replaced by "B", and that the labelling of the accompanying diagram is in reverse order. I do not excuse my sloppiness in proof-reading, but it seems to me that Williams is exaggerating the significance of my errors. My quotation is supposed to illustrate the beauty of Faraday's thought processes, not to clinch a crucial argument. Nothing is distorted to prove a point—which might with more justice be thought scandalous. And Williams acknowledges that sloppiness is a sign of no particular philosophical error. Why, then, is it worth publishing a photo-copy of my quotation with corrections indicated in pencil?

WILLIAM BERKSON
Washington, D.C.

WILLIAMS DODGES AGASSI'S CRITICISM

The 1975 fall issue of this *Journal* (**26**: 241–53) includes a paper by L. Pearce Williams, allegedly reviewing my *Faraday as a Natural Philosopher* and W. K. Berkson's *Fields of Force*, under the title 'Should Philosophers be Allowed to Write History?' Leaving the part on Berkson as none of my business, I wish to respond thrice to Williams's review: in the next sentence, in the first section of this reply, and in the rest of this reply. Williams corrects some minor 'errors of transcription' of mine, for which I am grateful; he foolishly pretends that historians can be and should be immune to such errors; he claims that my thesis is erroneous and unfounded whereas I have ample evidence, especially to refute his alleged refutation of it; and he preaches a preposterous self-refuting view which is summed up in 'a resounding "No"' to his question. End of my reply, shortest version.

1 Does Williams allow historians to write philosophy? Does he allow members of academic profession x to write in academic discipline y? I say "academic" since in the market place a few practices are confined by law to professionals, but not in the academies; not yet, at least. How Williams wants to ban intrusion—by law, by guild by-law, by editorial policy, or by reviewers' threat of a scowl—I would not know.

Should members of one academic profession be allowed to participate in another?

Suppose Williams says, NO. He thus advocates professionalism. Whose domain is the discussion on professionalism? Sociologists'? Philosophers' of science? Surely not historians'.

Suppose Williams only says NO to philosophers' meddling in others' affairs. Again, surely discussion of this prohibition is not a historian's domain.

Suppose Williams only says NO to meddlers in history. Anything peculiar to history? YES: philosophers care for ideas, not their origins; scientists offer conjectures and their attempted refutations. Only history is inductive and so both comprehensive and entirely founded on (all the available) evidence.

By the inductive historian's canons Williams is very much at fault in saying what I report him to say: Rather than generalise from one or two instances he ought, by his own lights (not by mine), to write a comprehensive history of scientists' and philosophers' writings on history. He could then find many excellent histories written by non-historians; he could likewise discover anti-inductivist excellent historians; he could then find no instance of an infallible historian. Some of our best friends, he and I know, are fallible, anti-inductivist, excellent historians, some of them strictly amateurs.

And so Williams refutes his thesis when he defends it, and it is a thesis that demands that it be defended. (This is known amongst philosophers as a pragmatic paradox.) Instead he should, like a good historian, explain why he still thinks Boscovich held a theory of space filled with forces when I tried to refute it by arguing that forces acting at a distance do not fill space and that Boscovich claimed that forces act at a distance. He could also explain how come so many of Faraday's correspondents whom I quote were Boscovichian anti-Faradayans. In his review he simply reiterates that Boscovich's and Faraday's views on force are the same; and honestly, I do not know what to do about this stubbornness.

End of second version.

2 The gist of Williams's first paragraph is this: 'Both Agassi and Berkson overtly make it clear at the beginning that they are using Faraday, in Agassi's words, "to illustrate a philosophy" and that philosophy is the one associated with Popper's name. This review will be directed at the question, what kind of history is written when it is "to illustrate a philosophy?"' (This is incorrect; see my reference to Emile Meyerson below.) Williams then discusses our 'errors of transcription' which, he admits, have nothing to do with his question. He then illustrates his question: he quotes a description of mine which 'serves Agassi's philosophical prejudices perfectly' but which is 'totally at variance with the facts'. After exposing a few more of my alleged errors, he says this: 'I have spent an inordinate amount of space pointing out these errors because Agassi has thrown out a challenge to scholarship that I think cannot be allowed to pass unanswered. Berkson seems to have followed Agassi and before their usage can swell into a trend, it must, I believe, be firmly blocked.' Now I do not know what the 'challenge to scholarship' is, how I threw it out, why it must be answered, and what usage of mine and Berkson's can 'swell into a trend'. But if he means merely to say that we have committed historical errors or, to be more precise, made some errors of transcription and some conjectures that Williams thinks are false or in need of better proof than we have for them, then now is the time to mention that Williams quotes my book in his motto to say I do not aspire so highly, and those who find my aspiration too low may read my book as a historical novel of sorts. Certainly, he cannot demand that

if I cannot write an infallible inductive history I should write no history and not even a historical novel; surely he can concede that my book is at least a historical novel for infallibilists though perhaps history for fallibilists? Even that may be too extravagant: even Williams's hero, the infallibilist Collingwood, could never bring himself to call fallible and erroneous histories no histories at all and worse than nothing.

3 Williams agrees with Collingwood, who 'is to be interpreted as meaning that the historian and especially the biographer must attempt, in so far as possible, to put himself literally in his subject's place.' Should we take 'literally' here literally? Otherwise, it simply means that a historian or biographer should empathise with his subject. Williams declares that I have not done so. He proves this by referring to my own admission that my story of Faraday comes to illustrate my philosophy and by (allegedly) showing me in error on various issues. His concluding paragraph makes it clearer: empathy allows no specific viewpoint: it is comprehensive and infallible.

This is all well and good, except that Collingwood has come to the wrong party: it is common knowledge that he thought comprehensive history is impossible since empathy brings certainty only to success stories, not to failures (of which Faraday's life was full) and since philosophic presuppositions are unavoidable, and even hypotheses are essential to research. Moreover, what should one—historian or not—do if one disagrees with both Williams and Collingwood? It is, to add, common knowledge that Collingwood never won public assent; how come now he appears to Williams as the right counsel and authority on history?

The penultimate paragraph of Williams's review explains his proposal to prohibit history from philosophers' pens: 'Philosophers tend to be interested in ideas . . . They do not seem to find it very interesting to ask where ideas came from, how they developed', etc. '. . . as we have seen, they are at their worst when trying to account for the evolution of one . . . they cannot really follow Collingwood's advice . . .'. And he concludes by saying, science can develop on Popperian lines, where one author offers conjectures and another offers refutations, but not history: 'History is an inductive science. There are a finite number of facts that pertain to Faraday available to the historian . . . any attempt to understand Faraday and discover him, in Collingwood's sense, . . . *must* take the whole body of historical facts on Faraday into consideration. The historian is not free to pick and choose those facts that suit his thesis . . . Nor is it legitimate to make statements for which there is no evidence whatsoever . . . If one does, the result is not falsifiability and progress, but falsehood and bad scholarship.' End of review.

Does Williams say that he has considered correctly all the facts on Faraday when he wrote his book on Faraday? That he had made no conjecture there? Any reader who thinks that perhaps the answer is in the affirmative is invited to look up my book and Berkson's and easily check our comments on Williams. Does Williams think that his view is the same as Collingwood's? If anyone thinks pêrhaps the answer is in the affirmative, they might easily learn that it is not so by any reasonable checking. Indeed, why do we need empathy at all if we have evidence to prove all that we say about our heroes?

4 One section I wish to devote to Faraday as a natural philosopher. I do not know if Emile Meyerson, whom I follow, was a historian or a philosopher, but he has noted that though Faraday said he was following Boscovich, he was not, since for Boscovich matter centred in point atoms interacting at a distance, whereas Faraday denied such action. I have discussed this at great length in my book. I explained that Laplacian fields of force are no more than mathematical devices because Laplace and all other Newtonians thought outside atoms space is strictly empty and, action between them does not occupy the intervening space. I explained why Faraday insisted that the fields are real and why forces travel in intervening space in time, that Faraday distinguished between real fields and representational fields that may turn out later to be real or fictitious. I explained why the Boscovichian de la Rive, Faraday's best friend, considered him an idealist: he thought the view of fields of force as more fundamental than the atoms of matter is dangerously idealistic sounding. I quoted Faraday's responses to some private letters on the controversy where he expresses his wish to see the debate made public; his responses to the conspiracy of silence of the learned world regarding fields and concerning his rekindled interest and revived memory when fields were finally discussed. Now Williams says that I am in error when I differentiate Boscovich and Faraday, that a glance at Boscovich's diagram should have corrected my error, that I have no evidence regarding my psychological characterisation of Faraday as vacillating between high hopes and deep despair, and that my story is sheer romantic fancy since (admittedly) Faraday was a member of the establishment.

5 Some of Williams's corrections of my errors—especially one or two of transcription—are accepted and welcome; with gratitude. I am sure my second edition should include an acknowledgment to that effect.

One last point. In his opening he has a note expressing unease about seeming to put down my book on Faraday so as to boost his: he protests he tried to be objective. Had he consulted me about this unease I would have recommended an easy remedy after criticising his remedy: I would have told him he could not put aside ('bracket' as philosophers say) the fact that he had written a book, quite apart from his own demand that a historian be comprehensive; I would have advised him to do the opposite of putting it aside. I would have advised him to respond, one way or another, to corrections to his book in mine (and to his book and to mine in Berkson's!). In any case, my parting shot is this. People who live in intellectual glass houses should certainly be given all encouragement to throw intellectual stones; but their pretence to be living in shatterproof, airtight, crystal-clear, comprehensive, faultless domes can be viewed as somewhat exaggerated.

Of the many mis-representations of Williams not central to our debate I pick up one: the course of Faraday's senility. Before Williams published his *Faraday* he told me he suspected Faraday suffered from mercury poisoning. I told him this was at best *ad hoc*, since many chemists worked on mercury and evaded senility; and, moreover, I had evidence against that idea: Faraday told Tyndall in a letter published in his official biography that seeing two papers that had made mention of his lines of force he found his memory refreshed and his ability to think revived, and he even decided, he added, to write a new paper or two. In his *Faraday* Williams does not discuss the causes of Faraday's senility. In his review of my *Faraday as a Natural Philosopher* he presents his old idea without referring to his book and without mentioning the evidence in mine.

I do not recommend censorship against one who violates a minimal standard—the standard that we should notice criticisms of our own views—and who preaches censorship in the name of the protection of a maximal standard—the standard that we should take account of every detail available. I understand that if he had everyone do things exactly in his way, he would feel very happy; and that it matters little to him that others might then feel less happy. But I suppose he can understand that some of us have our doubts.

JOSEPH AGASSI
Boston University and
Tel Aviv University

REPLY TO AGASSI AND BERKSON

I am perfectly content with Professor Agassi's description of his work as a historical novel. Might I suggest that, for the proposed second edition, he change the title so that his readers will not be misled into believing it a work of serious scholarship? How about *Forever Faraday*?

Dr Berkson has convinced me that I have done him an injustice in my review of his book, and I should like here to apologise for it. My scholarly ire had been raised very high by Professor Agassi's cavalier treatment of facts and texts, and this caused me to over-react to Dr Berkson's errors of transcription. Although I still feel that these are serious and not to be tolerated, I should like to add that they do not, in fact, affect his argument. His argument is an important and scholarly one that deserves to be taken seriously.

Much, if not most, of the disagreement between me and Dr Berkson arises from what I consider to be Dr Berkson's failure to think historically. He would find Faraday's germinal and fundamental ideas, full-blown, as early as 1831; I insist that they were confused and embryonic then and only grew gradually through the years to their full maturity in the 1850s. Where Dr Berkson and I differ over the nature of these ideas, I believe later scholarship will provide clarification and modification of what the two of us have written.

L. PEARCE WILLIAMS
Cornell University

A REPLY TO 'SOME NEW ASPECTS OF RELATIVITY: COMMENTS ON ZAHAR'S PAPER'

In this *Journal*, M. F. Podlaha [1976] prefaced his paper concerning ostensibly the "polemical analysis"[1] of K. F. Schaffner [1974] and myself [1974] of E. Zahar [1973] as follows[2]:

---

[1] Podlaha [1976], p. 261.

[2] *Loc. cit.*

> . . . the criticisms made by both Schaffner and Miller are wrong as regards their central points. Indeed, it seems to me that their mistakes are the result of a misunderstanding, both of the source material, and of Zahar's paper. I shall deal with each of these points in turn.

Podlaha's paper turned out, however, to be mainly a vehicle for stating his own views on alternatives to Einstein's special relativity theory. Since my critique of Zahar's analysis was of a historical nature, I need not comment upon Podlaha's own research. But I shall reply to his criticism as they concern directly the physics of 1905 and demonstrate that one may well have some doubts about both the level of historical scholarship and the care exerted in reading papers.

1 Podlaha writes[1]:

> I feel that the statement by Miller, that not only Poincàré but also Einstein in 1905 recognised the group character of the Lorentz transformation, is wrong. I cannot find any statements or discussions of these group properties in Einstein's 1905 paper.

Podlaha's reference number is to page 44 of my [1974] where I cited the relevant page of Einstein's seminal paper on the special relativity theory [1905]. Had Podlaha looked at this page in Einstein's [1905] he would have seen that Einstein concludes the deduction of the velocity addition law from considerations based upon two successive parallel relativistic transformations with the statement 'that such parallel transformations—necessarily—form a group'.[2] Furthermore, Einstein's proof of the group property of parallel relativistic transformations is referenced, discussed and compared with Poincaré's proof in my [1973], which is cited in my criticisms of Zahar.[3] It seems that Podlaha has not studied very carefully either Einstein's original paper on the special relativity theory or relevant secondary material by the author whom he claims to be criticising.

2 Podlaha writes[4]:

> Both Schaffner and Miller state that the Lorentz–Fitzgerald Contraction Hypothesis (*L.F.C.*) is *ad hoc*, whereas—and this is particularly emphasised by Schaffner—Einstein's light postulate possessed considerable independent support. For instance the constancy of the velocity of light (*C.L.V.*) is confirmed by the Michelson–Morley experiment. But the *L.F.C.* was not *ad hoc*.
>
> The *L.F.C.* was formulated to explain the result of the Michelson–Morley experiment. It could be considered *ad hoc* only if it had no independently testable consequences, or, at least, if there were no independent tests available at the time. However, such tests were available.

The 'tests' that Podlaha offers as evidence against the ad hocness of the L.F.C. are: the 'Trouton–Noble experiment of 1903'[5] 'the law of the variation of mass with velocity (experimentally verified in 1914)'[6] and the Kennedy–Thorndike experiment of 1932 which 'is logically equivalent to the Michelson–Morley experiment.'[1] As I demonstrated in my criticism of Zahar, and recently elsewhere,[2] no useful purpose is served by analysing philosophically an event in science out of its historic context. The indisputable fact is that when Lorentz first proposed the L.F.C. in 1892 and then again in 1895, in no uncertain terms he considered this hypothesis to be *ad hoc*; Poincaré criticised the Lorentz theory on just this point in 1900. By 1904 Lorentz no longer considered the L.F.C. as *ad hoc* because it could be used to discuss experiments other than that of Michelson and Morley—for example, the experiments of Trouton and Noble, Rayleigh, Brace and Kaufmann. Poincaré concurred.

---

[1] *Ibid.*, p. 266.

[2] Einstein [1905], p. 51.

[3] Miller [1973], p. 262.

[4] Podlaha [1976], p. 261.

[5] *Loc. cit.*

[6] *Ibid.*, p. 262.

3 Podlaha writes[3]:

> Miller's statement that '. . . a theory of light containing Lorentz's ether cannot account exactly for the optical Doppler effect', is wrong. Miller could have easily avoided this erroneous judgment if he had carefully studied the series of papers by Ives, appearing in the years 1937–48 in the *Journal of the Optical Society of America* or if he studied at least the paper by Robertson of 1949.

Contrary to Podlaha's opinion the papers of Ives and Robinson are well known. But perhaps it is good to restate the hypotheses upon which Ives based his version of the Lorentz–Larmor theory; the existence of a 'fixed ether', which can be identified as 'the seat of the pattern of radiant energy received from the fixed stars'[4]; it contains two sets of measuring rods and clocks, and one set is '*unaffected by transport*'[5]; the L.F.C. hypothesis; and the hypothesis that the velocity of light is exactly c ($= 3 \times 10^8$ m/sec) only relative to observers at rest in the ether who use the set of rods and clocks that is unaffected by transport. From these hypotheses Ives succeeds in deducing a result for the optical Doppler effect observed in an inertial reference system. However, the deduction requires a rather artificial limiting process in order to remove unmeasurable quantities. According to Ives the time it takes light to travel a distance d between two points a and b in an inertial reference system travelling at velocity v relative to the ether is determined by a clock that is moved from a to b at a velocity W, and the distance d is measured with a rod that moves past a and b at a velocity Y, where W and Y are velocities relative to the inertial reference system. In the limit that W and Y are reduced to zero, *i.e.*, infinitely slow transport of the clock and rod, terms containing the unmeasurable quantity v vanish, and Ives obtains the exact equation for the optical Doppler effect—namely, the result from Einstein's special relativity theory of 1905. On the other hand, Einstein could deduce this result from the two postulates of his special relativity theory[6] with neither approximations nor unmeasurable quantities appearing in the derivation.

---

[1] *Loc. cit.*

[2] Miller [1975].

[3] Podlaha [1976], p. 263.

[4] Ives [1937], p. 263. Needless to say, what exactly are the 'fixed stars' is difficult to define, and just this criticism happened to have been levelled against the Lorentz theory 23 years before Ives's deliberations appeared in print. See Silberstein [1914], p. 88.

[5] *Loc. cit.* Italics in original.

[6] Ironically, Ives' defence in [1950] of this limiting process and his manner of derivation is almost verbatim from Einstein's reply ([1949], p. 679) to Bridgman's operationalist critique of the special and general theories of relativity in the recently published Schilpp volume; Ives references this volume ([1950], p. 191). In this paper of 1950 Ives writes ([1950], p. 189):

I found Podlaha's reference to Robinson's paper [1949] to be very mysterious because Robinson discusses only the special relativity theory. In fact, Robertson has only the following sharply critical comment of ether-based theories of the electrodynamics of moving bodies circa 1905 *vis-à-vis* Einstein's viewpoint[1]:

> At the time this work [Einstein's] was done an inductive approach could not have led unambiguously to the theory proposed, for the principal relevant observations then available, notably the 'ether-drift' experiment of Michelson and Morley (1886), could be accounted for in other, although less appealing ways.

4 Podlaha writes[2]:

> Also Miller states that 'The inclusion by Lorentz of true length and time quantities which remain unknown to the observer in an inertial system, renders unclear the meaning of the contraction of a rod at rest relative to S by an observer in S' . . .'. This is wrong. On the contrary, as I have shown in detail elsewhere, the *real* contraction of a rod as well as the *real* slowing down of the clock have a quite clear physical meaning, and make it possible to build a quite consistent theory of space and time.
>
> In my [1975*a*] and [1974*b*] I have also shown that all relativistic phenomena have their origin only in the contraction of lengths and in the slowing down of clocks which are as *real* as the contraction of a material rod due to the decrease of temperature.

First this quotation is not completely correct; the latter part should read—'. . . relative to an inertial system S_{r}^{1} by an observer in an inertial system S_{r}^{2}'.[3] Second, Podlaha took this statement out of its context for the purpose of advertising what he claims to be his own 'quite consistent theory of space and time.' The context of my statement is that whereas in Einstein's theory the length contraction is a symmetrical phenomenon involving unambiguously known lengths, this is not the case for Lorentz's theory. The reason is that in Lorentz's theory there are preferred reference systems in the ether whose observers can measure true lengths; however, these observers cannot communicate their results to observers in the inertial reference systems $\mathrm{S}_{\mathrm{r}}^{1}$, $\mathrm{S}_{\mathrm{r}}^{2}$. . . . Therefore, the Einstein and Lorentz theories are not observationally equivalent as concerns the contraction of bodies in motion; Zahar claimed that they were observationally equivalent. I did not discuss whether the contraction of a rod in Einstein's theory is 'real'.

In summary, Podlaha's criticisms are either incorrect or beside the point, being mainly an advertisement for his own recent views on alternatives to Einstein's special relativity theory.

ARTHUR I. MILLER
University of Lowell

The philosophical issue here is that we are not violating operational principles by using conceivable although experimentally unidentifiable operations in our theoretical attack, provided our final equations represent only performable operations and measurements.

[1] Robertson [1949], p. 378. [2] Podlaha [1976], p. 263. [3] Miller [1974], p. 43.

REFERENCES

EINSTEIN, A. [1905]: 'Zur Elektrodynamik bewegter Körper', *Annalen der Physik*, **17**, 891–921; references to the reprint in *The Principle of Relativity*, translated by W. Perrett and G. B. Jeffery, pp. 37–65.

EINSTEIN, A. [1949]: 'Reply to Criticisms', in P. A. Schilpp (*ed.*): *Albert Einstein: Philosopher-Scientist*, pp. 665–88.

IVES, H. E. [1937]: 'Light Signals on Moving Bodies as Measured by Transported Rods and Clocks', *Journal of the Optical Society of America*, **27**, pp. 263–73.

IVES, H. E. [1950]: 'Extrapolation from the Michelson–Morley experiment', *Journal of the Optical Society of America*, **40**, pp. 185–91.

MILLER, A. I. [1973]: 'A Study of Henri Poincaré's "Sur la dynamique de l'électron",' *Archive for History of Exact Sciences*, **10**, nos. 3–5, pp. 207–328.

MILLER, A. I. [1974]: 'On Lorentz's Methodology', *The British Journal for the Philosophy of Science*, **25**, pp. 20–45.

MILLER, A. I. [1975]: Essay-Book Review of Adolf Grünbaum's, *Philosophical Problems of Space and Time*, *Isis*, **66**, pp. 590–4.

PODLAHA, M. F. [1976]: 'Some New Aspects of Relativity: Comments on Zahar's Paper', *The British Journal for the Philosophy of Science*, **27**, pp. 261–7.

ROBERTSON, H. P. [1949]: 'Postulate versus Observation in the Special Theory of Relativity', *Reviews of Modern Physics*, **21**, pp. 378–82.

SCHAFFNER, K. F. [1974]: 'Einstein Versus Lorentz: Research Programmes and the Logic of Comparative Theory Evaluation', *The British Journal for the Philosophy of Science*, **25**, pp. 45–78.

SILBERSTEIN, L. [1914]: *The Theory of Relativity*.

ZAHAR, E. G. [1973]: 'Why did Einstein's Programme Supersede Lorentz's? (I)', *The British Journal for the Philosophy of Science*, **24**, pp. 95–123.

GROUP SELECTION AND METHODOLOGICAL INDIVIDUALISM: A CRITICISM OF WATKINS

J. W. N. Watkins has recently offered a modified version of his theory of 'methodological individualism'.[1] Watkins's change of mind is partially motivated by what is known as the theory of group selection in evolutionary biology.[2] I shall argue that the theory of group selection as it now stands is intrinsically and incontrovertibly muddled; that it is, at best, merely a confused restatement of a problem (for better or worse, this problem is now known as the problem of the evolution of 'altruism'). If the theory of group selection is as weak as I claim, then it offers little or no motivation or support for Watkins's change of mind. Thus I would argue that Watkins's modification of methodological individualism is uncalled for, in so far as the theory of group selection actually motivates or supports that modification.

The outline of the argument is as follows. First is a description of the theory of group selection with special attention to Watkins's use of it; next is a demonstration that the basic ideas of the current theory of group selection are muddled, and that they intrinsically clash with the Darwinian theory of evolution. Finally, I shall look at several assumptions that are philosophically relevant to Watkins's individualism as these ideas are illuminated by the preceding discussion.

---

[1] Watkins [1976].

[2] The *locus classicus* for group selection theory in modern biology is Wynne-Edwards [1962]. An excellent summary of some more recent thought is given in Williams (*ed.*) [1971]. My critique is largely indebted to M. T. Ghiselin's chapter 'The Legacy of the Stagirite' in his [1974].

1 Watkins quite accurately describes both the problems leading to the theory of group selection and the answers given by the theory. The basic problem which instigated the theory of group selection is the problem of the control of animal population size and, as Watkins puts it, there is 'a wide variety of [such] phenomena which were well known but could not be easily accounted for in terms of individual selection and propagation'. Individual, or Natural, selection operates through the relative success or failure between individual organisms' natality and mortality; group selection operates through the relative success or failure between individual populational groups' natality and mortality. Whereas natural selection gives rise to *organic adaptations*, group selection gives rise to the so-called *biotic adaptations*.[1] Organic adaptations promote the genetic survival of individuals, biotic adaptations promote the genetic survival of some population of individuals.

According to group selectionists, the most fundamental of the biotic adaptations is *reproductive restraint*. Wynne-Edwards argues that 'animal populations are in fact normally smaller than they might be, smaller than resources might temporarily permit. [Wynne-Edwards] ascribes this situation to population control mechanisms operative in the animals themselves. He says, in effect, that animals as a very general rule practice birth control to an effective degree'.[2] Thus a phenomenon of importance to the group—population control—causes the evolutionary adaptation that manifests itself through individuals representative of the group—some form of reproductive restraint. Wynne-Edwards's point is that a limitation upon the reproductive success of an individual organism is apparently inexplicable as an organic adaptation which promotes the individuals' genetic success. Reproductive restraint is explained by Wynne-Edwards purely in terms of biotic adaptations and therefore he suggests a mechanism for the evolution of such biotic adaptations, and this mechanism is group selection, an evolutionary competition between populations of organisms.

There is an even more fundamental adaptation than reproductive restraint which may be biotic, this is the adaptation of sexual reproduction itself. Consider that asexual reproduction allows an individual's offspring to be close to 100 per cent genetically similar to the parent, but that sexual reproduction allows for at most a 50 per cent similarity to the parent. This is a two to one debit for the individual' which poses a fundamental problem for Darwinian evolutionary theory: 'How is it adaptive for an individual to reduce his genetic reproductive efficiency by two to one?'[3]

Marvellously, for he is a self-admitted amateur at biology, Watkins senses this fundamental difficulty inherent in sex. He summarises the aspects of the theory of group selection which he utilises in terms of sex. *Group selection*, unlike natural selection, ***explains sex as a restraint on individual reproduction for the good of the Species.*** Furthermore, group selection continues to provide reproductive restraints which are of social benefit. Watkins is very clear on all of these points:

---

[1] The terminology and analysis derive from Williams [1966].

[2] Williams [1971], p. 11.

[3] See Williams [1975], chapter one, for a concise account of the problem. Ghiselin [1974] treats the problem more philosophically and at greater length.

> group selection allows us to go further than Aristotle. It tells us that the pre-attunement of an organism's inherited endowment to the collective needs of the group or population to which it belongs is by no means confined to its sexual endowment. Indeed, as we saw, a main function of mechanisms evolved through group selection is precisely to restrain the operations of sexual instinct so as to check the growth of a population above the optimal level . . . I now hold that the evolutionary process has adapted human capacities to the needs of social existence.[1]

In short, Watkins sees human society as involving biotic adaptations, which apparently presuppose a mechanism of group selection instead of, or in addition to, natural selection. From this basis he modifies his previous individualism to the extent that biotic adaptations such as territoriality are said to exist for the good of a population or group of human individuals. These biotic adaptations lend a certain reality to some populational groups in the natural and social economy, a reality hitherto denied by Watkins's individualism. The above summarises Watkins; reliance on the theory of group selection in his modification of methodological individualism. Unfortunately, the theory of group selection was largely refuted by Williams[2] and made superfluous with Ghiselin's and Williams's thorough accounts of one alleged biotic adaptation, the evolution of sex (by means of natural, not group, selection).[3]

2 Group selection does nothing more than to restate the problem it has set out to answer: 'How do adaptations in individuals arise which seemingly benefit not those individuals, but the species or some larger group of individuals?' Group selectionists say that biotic adaptations arise because adaptations arise which favour species (or populations) one over the other—because species, *per se*, compete in the economy of nature. An immediate problem for group selectionists is showing why extinction should exist at all if there are adaptations for the good of species; however, most group selectionists avoid the difficulty of extinction by employing group selection sparingly—only when natural selection explanations do not seem evident (see Watkins's quote *above*, p. 2). This restrained use of group selection has in turn led to a curious situation in that natural selectionists have explained many difficult questions of adaptations which were first brought to light not as problems, but as solutions by group selectionists! (For example, sex, senescence, territoriality, sociality, poisonous flesh, *etc.*). All of the confusion derives, I believe, from a fundamental mistake the group selection theorists make concerning evolutionary theory.

A theory of evolution necessarily involves two main components. First, *one must describe what evolution is*; that is, one has to know what is to be explained. Second, one must provide *a mechanism to account for the previously described situation.* This mechanism can be analysed at many levels: natural and sexual selection were Darwin's primary evolutionary mechanisms, but he hoped also to substantiate those mechanisms further with a genetic mechanism. Most biologists would grant that Darwin established natural and sexual selection ecologically, but that it was not until Haldane, Fisher and Dobzhansky in the

---

[1] Watkins, *op. cit.*, p. 712.

[2] Williams [1966].

[3] Ghiselin [1974] and Williams [1971].

1930s that a physiological, or genetic, basis for the evolutionary process was found.[1]

Darwin described evolution as ***the development of material variations between organisms into material variations between populations of organisms*** (I am paraphrasing Lewontin).[2] Darwin, as was mentioned, proposed two mechanisms to explain this phenomenon on natural selection and sexual selection. Natural selection operates in a Malthusian fashion, organisms will necessarily compete for limited resources. Sexual selection is non-Malthusian because it is focused through the reproductive resources of organisms themselves, resources which are inherently limited. Any resource, limited or not, may be used by an individual to further its reproductive success. Darwin used natural selection to explain the evolution of organic adaptations, and he used sexual selection to explain the refinements found in nature of already established adaptations, especially behavioural ones.[3] Williams[4] has explained most biotic adaptations as being aggregations of organic adaptations, and Ghiselin[5] has explained the importance of sexual selection for the study of adaptation. Many of the so-called social adaptations are actually sexually selected refinements of organic adaptations (compare Ghiselin's account of society with Wilson's[6]).

The group selection theorist is not interested in Darwinian evolution, for he is concerned with the development of variations between individuals as arising from differences between groups—the precise opposite of Darwin's description of evolution. Moreover, group selection is not a mechanism for this non-Darwinian form of evolution, but merely a rephrasing of the alleged phenomena to the effect that differences between groups will produce differences between individuals. *If* group selectionists were to recognise that they are proposing a second form of evolution, and *if* they further attempted to develop an explanatory mechanism appropriate for such evolution, ***then*** the theory of group selection could provide a basis for understanding evolution or society. Unfortunately, all group selectionists so far have attempted merely to graft a mechanism of group selection onto the Darwinian description of evolution, and this has led to much confusion (*e.g.*, the 'sociobiology controversy'), because the mechanism is unrelated to Darwinian evolution.

Finally, both Ghiselin[7] and Williams[8] have convincingly demonstrated that the phenomena of sexuality, altruism, population control, territoriality, *etc.* are all as well explained through the mechanism of natural selection as by group selection. If this is the case (and no one doubts that their accounts of the evolution of sexuality are the best yet given) then the development of a group selection theory seems unmotivated. Williams[9] argues that a mechanism of group selection is unnecessary because all phenomena alleged to be biotic adaptations can be (and have been) subsumed as organic adaptations, and that parsimony would suggest that Darwinian evolution is therefore sufficient.

---

[1] Ghiselin [1975].

[2] Lewontin [1974], p. 4.

[3] These issues were originally discussed, and quite thoroughly, in Charles Darwin's two most famous books, *The Origin of Species by Means of Natural Selection* ([1859]) and *The Descent of Man and Selection in Relation to Sex* ([1871]).

[4] Williams [1966].

[5] Ghiselin [1974].

[6] Ghiselin [1971–2] can be compared to Wilson [1975].

[7] Ghiselin [1974].

[8] Williams [1966].

[9] Williams [1966].

3 In their separate critiques of the theory of group selection, Ghiselin and Williams both point out that group selection is another version of the metaphysical argument from design. The metaphysics of design was propounded especially by the natural theologians such as Paley[1] who suggested that God had designed all creatures, for otherwise they could not be so well adapted. Darwin criticised the metaphysics of design at great length, proposing a design-less metaphysics and demonstrating its biological use by showing that one need not suppose any design to explain adaptations.[2] Group selectionists, like the natural theologians, disagree with Darwin's metaphysics, they simply cannot believe that natural and reproductive selection can explain adaptations. Consequently, group selection is invoked *ad hoc* to cover any apparent problems of natural selection theory. Specifically, group selectionists propose that *some* adaptations are best explained as *being for the good of some group* of organisms. The modern Darwinians argue that this is merely an *ad hoc* account in which *some individuals are seemingly designed to fit into a group pattern*,[3] Williams candidly admits that no one can very well describe a group or species in nature, so how, he asks, can one test the group selectionist claim that something is for the good of the species?[4] Williams therefore contends that all group selectionist claims are *ad hoc* in that whatever is needful of explanation is called 'for the good of the species' and that no independent test of such goodness has been developed. Finally, a group selectionist starts with the reality of a populational group (*i.e.*, over and above being a 'mere aggregation') and must therefore attribute some ecological reality to groups *per se.* Most group selectionists have utilised the notion of gene pool to secure ecological reality for their groups but the gene pool is a theoretical concept for which no ecological reality has been, or could be, claimed.[5] (Group selectionists would also have to use some notion of a 'phenotype pool' anyway, for selection operates through phenotypes, not genotypes).

In a letter dated 14 July 1977 to this author, Watkins comments that the recent shift among some sociobiologists away from group selection *per se* towards such theories as that of 'Evolutionary Stable Strategies' in terms of 'selfish' genes renders 'both my ideas and your criticisms very much out of date'. (*Cf.* for example, Dawkins's *The Selfish Gene.*) While it is somewhat rash to assert of a new theory that it is not significantly different from an old one in certain respects, I think that this can be said of any modification of group selection theory that appeals to genes. Darwin's theory of evolution by natural selection is, as was stated above, a theory about how variations among individuals are transformed into variations between groups of individuals. According to Lewontin this description of the theory is sufficiently formal so that it would apply to any variety of individuals, be they genes, cells or organisms. The problem with group selection theory, whether or not couched genetically, is that it is a theory as to how variations between groups are transformed into variations between individuals. *Prima facie* there would be no reason to suspect that such a theory might not work; however, in the century and a quarter since Darwin's *Origin* an overwhelming amount of evidence has accrued against such a theory. Moreover, with the recent publications by Williams, Ghiselin and others discussed above, no very cogent theoretical reasons remain motivating a theory of group selection.

---

[1] Paley [1836].

[2] Darwin [1876]. This book, originally written three years after the publication of The *Origin* has been described by Ghiselin, in his [1969] as a biological *Candide.* This book on Orchids was written by Darwin to satirise the argument from design, and also, along with The *Variation* book to refute Asa Gray's hypothesis that natural selection might proceed in orderly or designed 'streams', a theory of which group selection might be considered a variant. Darwin was disappointed in the response to this work, commenting to Gray that he (Gray) was the only reader who perceived that the book was a 'flank attack on the enemy'. Modern readers are still fooled, witness Ghiselin's barb that one misread the book so thoroughly as to make Voltaire out to be a Pangloss.

[3] Ghiselin [1974], chapter 2.

[4] Williams [1966].

[5] Wilson [1975], is the primary instigator of genetic accounts in modern group selection and sociobiological theory, in Dawkins [1976], there is an attempt to set the group selection matter straight, by appealing to genetics. Dawkins rightly criticises group selectionists for (*a*) presenting an unlikely theory and, (*b*) often dressing that theory up as Darwin's theory (which it is not). Dawkins then falls prey to a Butlerian teleology, speaking of 'selfish genes' and strategies of genes. (Does the chicken make more chickens with eggs, or does the egg make more eggs with chickens? See Ghiselin [1974], p. 38 for a thorough critique of this Butlerian fallacy.) As I suggest at the end of this paper, appealing to genetics is not enough to 'save' group selection as a viable evolutionary theory. So far as we know, it was Darwin's insistent individualism and anti-teleology which enabled his theory to work so well, and there is no evidence that revisions are necessary.

In short, group selection theory has both holistic and semi-teleological assumptions at its core. These assumptions conflict radically with Darwinian individualism and the causal mechanism of natural selection. Group selection assumptions and explanations have usually been brought in in an *ad hoc* manner in areas where Darwinian explanations seem in some difficulty, but it has been the general case that natural selection explanations were soon forthcoming (*e.g.*, for sex, senescence, altruism). This conflict of assumptions and disinterest in developing a theory totally independent of Darwinian theory has led to the theory of group selection being fundamentally confused as it now stands. Perhaps it would not be impossible to develop an adequate group selection theory in conflict with, or in addition to, Darwinian theory, but this has yet to be done.

Thus, I would recommend to Professor Watkins that he change his mind back to his original individualistic position. As things now stand, the biological evidence and theory which at least partially motivated and supported his shift appear to be totally ephemeral.

EDWARD REED
Boston University
Edinburgh University

REFERENCES

DARWIN, C. [1859]: *The Origin of Species by Means of Natural Selection.* 1st ed London: Murray.

DARWIN, C. [1871]: *The Descent of Man and Selection in Relation to Sex.* 1st ed. London: Murray.

DARWIN, C. [1876]: *The Various Contrivances by which Orchids are Fertilized by Insects.* 2nd ed. London: Murray.

DAWKINS, R. [1976]: *The Selfish Gene.* Oxford: Oxford University Press.

GHISELIN, M. T. [1969]: *The Triumph of the Darwinian Method.* Los Angeles: University of California Press.

GHISELIN, M. T. [1971–2]: 'The individual in the Darwinian revolution', *New Literary History*, 3, 113–4.

GHISELIN, M. T. [1974]: *Economy of Nature and the Evolution of Sex.* Los Angeles: University of California Press.

GHISELIN, M. T. [1975]: 'The rationale of pangenesis', *Genetics*, **79**, pp. 47–57.

LEWONTIN, R. C. [1974]: *The Genetic Basis of Evolutionary Change.* New York: Columbia University Press.

PALEY, W. [1836]: *Natural Theology.*

WATKINS, J. W. N. [1976]: 'The human condition: Two criticisms of Hobbes' in Cohen, Feyerabend and Wartofsky (*eds.*): *Essays in Honor of Imre Lakatos.* Boston: D. Reidel.

WILLIAMS, G. C. [1966]: *Adaptation and Natural Selection.* Princeton: Princeton University Press.

WILLIAMS, G. C. (*ed.*) [1971]: *Group Selection.* Chicago: Aldine.

WILLIAMS, G. C. [1975]: *The Evolution of Sex.* Princeton: Princeton University Press.

WILSON, E. O. [1975]: *Sociobiology.* Cambridge, Mass.: Harvard/Belknap.

WYNNE-EDWARDS, W. C. [1962]: *Animal Dispersion in Relation to Social Behaviour.* Edinburgh: Oliver & Boyd.

A PECULIAR CONSEQUENCE OF NICOD'S CRITERION

Nicod's sufficient condition for confirmation is the thesis (N) that any hypothesis of the form '$(x)\ (\psi x \supset \phi x)$' is confirmed by statements of the form '$\psi k\ \&\ \phi k$'. Discussion of this principle has centred upon the notorious 'paradoxes' of confirmation which it engenders. In particular, observation of a non-black non-raven would confirm 'All non-black things are non-ravens', and consequently, confirm the logically equivalent hypothesis 'All ravens are black'. And this seems odd.

There is however, an unrelated and still more bizarre effect of Nicod's criterion, compared with which the familiar 'paradoxes' are quite innocuous. It follows from (N) that, for any property P and pair of objects a and b, the observation that a and b each *lack* P confirms the hypothesis that a *has* P and the hypothesis that b *has* P.

To demonstrate this, consider

$$(x)\ [(\neg Px\ \&\ \neg Pb) \supset x \neq b]. \tag{1}$$

From (N) we may infer that (1) is confirmed by

$$\neg Pa\ \&\ \neg Pb\ \&\ a \neq b. \tag{2}$$

But (1) is logically equivalent to

$$Pb. \tag{3}$$

Therefore (2) confirms (3). Similarly, the hypothesis

$$(x)\ [(\neg Px\ \&\ \neg Pa) \supset x \neq a] \tag{4}$$

is confirmed by (2), and is logically equivalent to

$$Pa \tag{5}$$

Therefore the proposition '$\neg Pa\ \&\ \neg Pb\ \&\ a \neq b$' confirms both '$Pa$' and '$Pb$'.

(To see that (1) and (3) are equivalent, instantiate b in (1) to obtain $\neg Pb \supset b \neq b$, which entails (3); then note that if (3) is true, the antecedent of (1) is always false, so (1) is true.)

Here I have taken for granted the Equivalence Condition: if e confirms H and H is logically equivalent to J, then e confirms J. But there is a criticism of (*N*), implicit in the example under consideration, which does not require this assumption. Although (2) entails the ***negation*** of (1), it follows from (*N*) alone that (2) ***confirms*** (1).

An analogous difficulty is presented by

$$(x)\ [(Px\ \&\ Pb\ \&\ \neg Qb) \supset Qx]. \tag{6}$$

This should be confirmed by

$$Pa\ \&\ Pb\ \&\ \neg Qb\ \&\ Qa. \tag{7}$$

But (6) is logically equivalent to

$$\neg Pb \vee Qb. \tag{8}$$

Therefore (6) is inconsistent with the alleged evidence for it.

Thus our data may, according to Nicod's criterion, confirm some hypothesis—and yet entail that the hypothesis is false.

I should add that the formulation, (*N*), derives from Hempel's excellent discussion of Nicod's criterion in ***Aspects of Scientific Explanation***. It should not be supposed that Nicod himself would have accepted this formulation of his claim. Hempel shows that difficulties, related to those described above, will emerge if Nicod's criterion is extended and applied to universal conditional hypotheses in more than one variable. He suggests, however, that the restriction to a single variable would be sufficient to avoid them.

PAUL HORWICH
Massachusetts Institute of Technology

Brit. J. Phil. Sci. 29 (1978), 265–283 *Printed in Great Britain*

Review Articles

IS MIND AUTONOMOUS?*

Is the universe a causally closed system or, in the case of certain living organisms at least, do we have to reckon with the intervention of some extra-physical influence which, for convenience, we may here call mind? It would be hard to conceive of any more fundamental question than that even if there are those who are so dogmatic as to assume that we already know the answer, so that the question is no longer worth asking, or those who are so obtuse as to deny that the question itself is legitimate. Those who defend the 'closed-system' answer I shall hereinafter refer to as 'materialists', those who question the self-sufficiency of the physical world or accept the 'open-system' answer I shall hereinafter refer to as 'mentalists' or 'interactionists'. In this remarkable volume a great philosopher and a great physiologist come together to present a case for interactionism. This is a courageous stand inasmuch as interactionism is not fashionable with philosophers, some of the most influential contemporary philosophers having opted for one or another version of materialism, and is even more strongly resented by most of the brain scientists and cognitive psychologists of the new generation.[1] We may confidently anticipate that the book will meet with a barrage of criticism. For my part, however—and I think a reviewer should declare his interest from the outset—I have long been a convinced interactionist and can therefore sympathise with what these authors are trying to do. Accordingly, I shall be less concerned to question their arguments as to consider whether they could have made their case even more convincing. But before I get round to expressing my reservations I shall offer some general observations of a descriptive kind and attempt a brief exposition of some of the main theses that are here put forward.

The work consists of three parts, Part I is written by Sir Karl Popper, Part II is written by Sir John Eccles, while Part III comprises a set of dialogues between these two authors. Those who are familiar with their previous writings will not find, I think, that they here have anything radically new to suggest; the pretext for the venture lies rather in the juxtaposing of their very different contributions and in the ensuing confrontation between them. The volume may also be regarded as a happy object-lesson in the cross-fertilisation between science and philosophy which has always been a salient feature of the Popperian approach. The first part also represents much the fullest statement which Popper has yet given us on the mind-body problem, including, in his Chapter 5, an extraordinarily illuminating history of the problem from pre-Socratic times, and it is, moreover, the nearest that Popper has ever come to presenting a fullyfledged philosophy of mind. I need hardly add that it is beautifully written with all the characteristic clarity and absence of pedantry, and it is written, moreover, in a tone that exactly matches the message that it is trying to convey, namely that everything we say is a conjecture and that where truth is concerned there can be no ultimate authorities. It is a tone that is modest and tentative, without being apologetic, and there is no trace of the rancour that so often mars philosophical polemic. The author's attitude towards his opponents can only be described as chivalrous to a degree and he is always gracious in acknowledging others who have put forward similar ideas no matter how obscure these others may be.

---

* Review of Karl R. Popper and John C. Eccles [1977]: *The Self and Its Brain: An Argument for Interactionism.* Berlin/London: Springer International. £9.40. Pp. xvi+597.

[1] A recent series of Reith Lectures by a brain physiologist who has the distinction of being the youngest Reith Lecturer to date, expresses an almost militantly materialist approach, see Blakemore [1977].

With Part II we are transported to a very different universe of discourse. What we have here is a technical and highly condensed treatise on brain physiology complete with a multitude of elaborate diagrams. However, Professor Eccles is not just trying to blind us with science, it is the necessary prelude to the author's theories about the mind–brain liaison and, more especially, about the most likely loci of the interaction, what he calls the mind–brain interface. For, if we are going to take interactionism seriously and literally, then there must be some definite set of cells in the sensory and associated areas of the cortex from which the mind 'reads off' the required information during perception and another such set of cells in the motor cortex whose activities can be modified by the mind during voluntary action. Eccles must be among the few physiologists since Descartes to risk committing himself to any definite view about the mind–brain interface but, in doing so, he contrives to give a concreteness to the argument which Popper has developed along more general and abstract lines. In this way these two contributions complement one another even though their viewpoint and outlook is by no means identical. Eccles, one could say, is closer to Descartes than to Darwin. Thus he invariably speaks of the 'self-conscious mind', meaning by this the human mind, and expresses a positively Cartesian agnosticism about what sort of a mind if any is possessed by animals, almost conceding at some points that animals just might be nature's automata. He is also inclined to attribute a supernatural origin and perhaps a supernatural destination to the self-conscious mind (soul?) of the individual, all of which is far removed from the thinking of his partner. However, the inclusion of the dialogues of Part III provides an excellent opportunity for the two authors to sort out their differences in an amicable spirit as well as to discuss the application of their joint beliefs and theories to a variety of problems in human psychology including language, perception and memory.

I will now attempt to expound briefly what I take to be the gist of the Popperian thesis. I discern two distinct but interlocking ideas that give it its special character. The first is the conception of mind as a source of activity; Popper even says at one point (p. 538) 'I want to conjecture that selves are the only active agents in the universe: the only agents to whom the term activity can properly apply'. This immediately places it in opposition to the older empiricist or associationist theories of mind where the self was conceived either as the passive recipient for incoming sense-impressions or as a mere arena for the automatic couplings of ideas that found their way in. Indeed, Popper sees the whole development of associationist psychology as a misguided attempt to model the mind on the behaviour of matter in Newtonian physics and he attributes this error to the influence of Cartesian dualism as modified by later parallelistic ideas. For the same reason Popper will have no truck with modern materialist views of mind as propagated by the identity theorists like Feigl which would make of mind a mere private reflection of certain brain states and processes with no autonomy of its own. The second key idea we find in Popper is his belief in the universality of the Darwinian principle of trial and error not only in nature but in man's socio-cultural development. Popper sees evolution not as the blind mechanistic process it is sometimes made out to be but as a record of nature experimenting with successive problems. He takes his stand with certain recent interpreters of Darwinism according to whom random variation within a species is not the only basis on which natural selection operates, some credit is due to the habits and preferences acquired by individual members of the species in selecting their environments.[1] Thus, even at this stage we must recognise a continual give-and-take between an organism and its environment 'in a sense one could say that animals partly create themselves' (p. 566). Thus, once again mind as a source of activity is vindicated.

What ultimately brings mind into the forefront of the picture and allows for the emergence of the self-conscious mind of man is the creation of 'World 3'. Since Popper's three-tiered ontology forms one of the principal unifying themes of this book, shared alike by both its authors, I must now say a few words about it for the benefit of any reader not yet fully familiar with this important construct. World 1, the only world acknowledged by the materialist-monist, is just the physical world. World 2, which somehow emerges out of World 1 at a particular stage of evolution, even if we cannot pinpoint that unique event, is the world of primitive consciousness and its attendant mental events and entities. World 3, which emerges out of World 2, or, more accurately out of the interplay of World 2 and World 1, probably at the stage corresponding to the invention of language, is what, in plain English, we would call 'culture' as opposed to 'nature'. It is given a peculiarly Popperian twist, however, which justifies this introduction of a technical term, by being made the repository for all our ideas, theories, problems, everything, in short, which comes under the heading of what Popper calls 'objective knowledge'. Popper insists, again and again, that, even though in one sense the entities of World 3 are abstractions (except in so far as they are embodied in, or encoded in, World 1 objects), they possess an autonomy of their own which cannot be gainsaid; World 3 can no more be reduced to World 2 or World 1 than World 2 can be reduced to World 1 as the materialists would have us believe. Indeed the closest that Popper gets to a direct refutation of materialism is to argue that it cannot admit the existence of World 3 and hence cannot justify itself rationally since reason and argument themselves belong to World 3.

One of the central theses of the book is that the self, as distinct from mere mind or mere consciousness, is firmly anchored in World 3. A conjecture is made that, in the evolution of man, the growth of the brain and the development of language may well have proceeded hand in hand, each facilitating the other, and that perhaps something similar happens in the case of each individual brain when we acquire the use of language in infancy. If this conjecture is valid then we would have here an example of an interaction between a World 2 entity (a mind) and a World 3 entity (language) resulting in the modification of a World 1 entity (a brain). We learn in the course of the dialogues (p. 473) that the book was originally to have been called 'The Self and the Brain' but, in deference to the active nature of the self in determining the ultimate structure of the brain, this was changed to the present title 'The Self and *Its* Brain'.

---

[1] Notably Hardy [1965] and Mayr [1963].

I will try next to summarise Eccles's contribution. This is more difficult because while Eccles is very clear and explicit in spelling out his conception of the mind-brain relationship and is, indeed, exceedingly bold in speculating about its details, the arguments by which he reaches his conclusions are not easy to follow. We are confronted with such a density of technical information and we are exposed to so many devious and learned digressions that it is all too easy to lose the thread. Nevertheless, I will do what I can to reconstruct what I take to be the bones of the argument. First the author is at pains to show us that, whether or not there *is* a ghost in the machine, the machine itself is precisely of the kind one could reasonably expect a ghost to operate! Thus, thanks to the prodigious interconnectedness of the neuronal networks of the brain and to the delicately poised state of the brain mechanisms, even a very slight tampering with the otherwise automatic workings of the brain will escalate to produce decisive differences in gross behaviour. Eccles introduces us to the vertical 'modules' or columns of neurones in the cortex which have recently been discovered and which appear to function somewhat like the integrated microcircuits of the electronic engineer. These, he reckons, would be peculiarly well suited to serve as what he calls the 'liaison brain'; from scanning the spatio-temporal patterning of these 'open modules' the self-conscious mind would read off the information from the external world. Similar 'open modules' in the motor cortex would allow the willing of a muscular movement to translate itself into a discharge of the appropriate pyramidal cells.

But what evidence have we that there *is* such an entity as we are calling the self-conscious mind to exploit these possibilities of the brain for its own purposes? Eccles discusses in some detail two findings from recent brain research which, he argues, cannot be fully explained in purely physiological terms. The first pertains to some highly ingenious experiments by B. Libet the upshot of which is that, when a given train of signals from a weak repetitive stimulus to the skin reaches the somaesthetic cortex and eventually gives rise to a conscious experience of being touched, it is apprehended as if it had been experienced a few moments earlier, it is 'antedated'. This suggests a temporal analogue to the way in which, in normal vision, the signals from the eye which arrive at the visual cortex are spatially projected by the mind and perceived as an object in the external world. The second is the work of H. Kornhuber who studied the build up of a 'readiness potential' in the motor cortex that precedes voluntary movement. The subject's task, in this instance, was simply to wag his finger periodically while being otherwise in a totally relaxed state, a condition not easy to achieve without plenty of training. Eccles argues that, in the Kornhuber case, we are witnessing a pure example of the mind acting on the brain and insists that, neither in the Libet case nor in this case, is any purely physical explanation possible.

But what, I think, counts with Eccles more than anything else is his conviction that no purely physiological analysis of brain function could ever account for the unity we find in experience or for the unity of personality which makes us want to assign our varied experiences to a persistent self.

Perhaps only a quotation at this point can do justice to the claim which the author is putting forward:

> Hitherto it has been impossible to develop any neurophysiological theory that explains how a diversity of brain events comes to be synthesized so that there is a unified conscious experience of a global or gestalt character. The brain events remain disparate, being essentially the individual actions of countless neurones that are built into complex circuits and so participate in the spatiotemporal patterns of activity. This is the case even of the most specialized neurones so far detected, the feature detection neurones of the inferotemporal lobe of primates. Our present hypothesis regards the neuronal machinery as a multiplex of radiating and receiving structures: ***the experienced unity comes, not from a neurophysiological synthesis, but from the proposed integrating character of the self-conscious mind.*** We conjecture that in the first place the self-conscious mind is developed in order to give this unity of the self in all its conscious experience and actions (p. 362; author's italics).

This, then, in brief, is the Popper–Eccles case for interactionism. Will the materialist hordes be put to flight? It does not require much imagination to anticipate the lines along which the counterattack will be mounted. The authors will be openly accused of cheating, of inventing a bogus entity to which can be ascribed whatever powers are needed to produce, however miraculously, the phenomena we have got to explain. Their self-conscious mind, it will be said, which is continually extracting information from the brain or controlling its output is but the latest version of that homunculus from which scientific psychology has, from its inception, striven to free itself. Our task, instead, should be to propound models of mind, of mental functions, which can be tested by using them to programme a computer that would then simulate the relevant behaviour.

How vulnerable are the authors to this kind of criticism? Undoubtedly their conception of mind raises awkward problems as to how much autonomy we are supposed to ascribe to it. It would be hard to make any sense of what they are claiming, however, unless the self conscious mind were credited with certain very basic mental functions, say of searching, willing, apprehending *etc.*, which were logically prior to its engaging with the cerebral machinery and, hence, of a psychical rather than a psychophysical nature. But is this intolerable? Nature, after all, does not exist for the convenience of scientists. Suppose, for the sake of argument, that it *were* the case that the mind possesses certain powers of instantaneous global or gestalt recognition of a sort that could not be explicated or defined in terms that could be made the basis for simulation by a computer or Turing Machine. Would we be justified in glossing over this fact or pretending it was otherwise? One has only to pose the question to appreciate that our first commitment must be to the truth as we see it, not to any particular research programme however successful it may have been hitherto. Nevertheless, I wonder whether either author fully realises what a profound departure they are asking us to make with the whole trend of science as we have known it in post-galilean times with its reductionist and analytical aims. For these mental powers about which they speak are as ultimate or irreducible, in their own way, as, say, the force of gravitation in physics. At the present time when biology has made such unprecedented advances by adhering to the mechanist-reductionist approach it is unlikely that brain scientists will calmly consent to call off their pursuit of a physical theory of memory, intention or other such mental function in deference to a dualistic metaphysics. Rather they will persist in viewing the mind as the ultimate goal, the glittering prize, of a unified and all-embracing materialist science.

I wonder, too, whether Popper's attempt to present interactionism in an evolutionary framework is not a forlorn bid to make it palatable to the scientific community? For the whole evolutionary story including (pace Popper) the evolution of World 3 could, it seems to me, be expounded in terms compatible with panpsychism, parallelism, epiphenomenalism or any of the rival theories of the mind–body relationship. It is true that mind would no longer play any part in that story beyond mirroring what was taking place but this is not to say that the events in question could not have occurred. Moreover these rival accounts dispense with the problem of how mind originates whereas Popper has to suppose that consciousness suddenly arose out of nothing, or out of the totally incommensurable world of matter, and did so at just the juncture where it was needed if evolution was to continue. To call it one of nature's inventions and to compare it with the emergence of flight in the archaeopteryx, or even with the emergence of life itself, does not allay one's intellectual discomfort if only because it is so hard to see how consciousness could have arisen by trial and error. However, since there is no more plausible account of the origin of mind, Popper is entitled to his concept of 'emergence' whatever that may mean.

My main worry, however, concerns the way in which the key term 'interaction' has been deployed in this 'argument for interactionism'. The critical issue, as I suggested at the beginning of my review, is whether World 1 is a 'closed' or an 'open' system and, if the latter, what is the nature of the influence to which it is open? This issue, however, tends to be obscured here by Popper's preoccupation with his trialist ontology. For this yields two different types of inter-world interaction, that between World 1 and World 2 and that between World 2 and World 3. As Popper puts it: 'It [World 3] really does exist; and not only does it exist but it is active; it acts upon us (only, of course, by way of interaction). I conceive the relationship between World 1 and World 2 as being similar' (p. 563). Yet, if we take the term 'interaction' literally, we may define it as a two-way causal process between two distinct entities. Now, the entities of World 3 are, as Popper clearly states, timeless abstractions. How, then, can a timeless abstraction actively participate in what, by definition, is a temporal process? The answer, surely is that it can not and, indeed, when we look at what Popper means by an interaction between World 2 and World 3, we see that he is referring to some cognitive act in which a mind *grasps* some abstract object of thought, in other words to a mental event, anchored in World 2 although dependent, presumably, on events in World 1 (*i.e.* the brain).

Of course we could take interaction in a non-literal sense as when an historian or a sociologist might discuss the interaction between two different cultures or the interaction between Church and State. This usage is unproblematical, however, because we know that what is being talked about here is a whole class of specific causal transactions. In like vein, one could say of this review that it was a product of an interaction between the book and the reviewer. But, when Popper speaks about the interaction between his three worlds, one is genuinely puzzled as to whether this is to be understood literally or figuratively. I would be loath to scrap Popper's three world hierarchy, I have already found it an immensely valuable heuristic device for clarifying one's thinking over a wide range of issues, I would even accept Popper's contention that World 3 entities are, in a sense, autonomous, since no universals, after all, are ever reducible to particulars as a naive positivism would demand. But this is not the autonomy that I wish to ascribe to mind conceived as an ultimate power in nature on a level with the forces of physics. Perhaps, the root of this difficulty is that this three-world classification is offered as a contribution to ontology whereas it should be taken as a contribution to epistemology. At any rate, in my ontology there are (*a*) physical events and entities (*b*) mental events and entities and (*c*) nothing else (which does not mean, of course, that I refuse to recognise the epistemological significance of abstractions).

I come, finally, to my point of divergence from both these authors. Eccles repeatedly describes his own position as that of a 'strong dualism'. My complaint, quite simply, is that his dualism is not strong enough to serve as an adequate counterweight to the enormously powerful case for materialism which for all their concessions, both authors, I believe, underestimate. Thus, anyone arguing in favour of the autonomy of mind must reckon with the devastating evidence which science has accumulated pointing to the utter helplessness of mind and to its one-sided dependence on the intact brain. To take an example of such evidence (not mentioned here), consider the case-studies reported by Oliver Sacks [1973] of his patients who were victims of the epidemic of sleeping-sickness (*encephalitis lethargica*) that spread across the world after the First World War as a result of which they led thereafter a semi-vegetable existence in hospital until he recently began treatment with the drug L-Dopa. Like so many Rip Van Winkles they then carried on more or less from the point where they had left off as if nothing that had happened in the interim had any significance for them. What, we may well ask, was the autonomous self-conscious mind doing during those fifty or so wasted years? One could multiply such examples but I will mention only the case of the split-brain patients to which Eccles devotes an entire chapter (E5). Here we are confronted with the fact that, following a commissurotomy (a surgical transection of the *corpus callosum*) one part of the individual, that part that has the power of speech, which is dependent on the left hemisphere, evinces no awareness of signals reaching the right hemisphere although, that these *are* duly recognised, can be shown if the individual uses a non-verbal response governed by the right hemisphere, *e.g.* pointing with his left hand. Eccles's attempt to reconcile such facts with his belief in the unity and autonomy of the self-conscious mind is, to say the least, tortuous; roughly he has to suppose that, following the operation, the self-conscious mind takes up its abode in the articulate left hemisphere leaving the mute right hemisphere to function like an automaton. Yet, whatever may be the correct interpretation of what is going on in this strange situation, there can be little doubt that the materialist formula—two brains therefore two minds—is the simplest.

Granted, then, that materialism has in its favour both this evidence from the effects of brain lesions plus the fact that it is more consonant with the conventional scientific approach, what would be required to challenge its supremacy? The mind-body controversy has now gone on so long that it would be too much to expect that some new philosophical argument could do the trick. On the other hand, if we could find unequivocal evidence of mind acting independently of brain then materialism would, one may suppose, be thrown on the defensive as now interactionism is on the defensive. The question is: where are we to look for such evidence? I cannot believe, as Eccles seems to suggest, that we shall find it by looking still more closely at the brain. Whatever clever brain experiments he may cite which suggest to him a case of mind acting independently of brain, we may be sure that the heirs of Turing who are wedded to the idea that men are machines will always insist that with just a little more knowledge about the brain and the way it is programmed these experiments, too, will be explained without invoking any occult entities. On the other hand, if mind has the kind of autonomy which Eccles says it has, then it is at any rate theoretically possible that the self could also interact with objects *other than* its own brain.

But, is there any evidence that such a thing ever actually occurs? Or must we resign ourselves to the fact that nature has so arranged things that mind can function only in conjunction with the one object with which it has symbiotically developed? The answer, as we all know, is that there is indeed a mountain of such evidence if we choose to examine it. It is not, of course, conclusive evidence of anything; it is not even good evidence by any standard scientific criterion, but it is evidence none the less and there are several passages, particularly in the Eccles section of the book, which cry aloud for some acknowledgment that it exists. Thus, Eccles speculates at one point (p. 372) as to what the self-conscious mind might be trying to do when the brain was in a state of deep anaesthesia or coma. But he drops no hint that there is a fair amount of evidence that sometimes the mind appears to be fully alert and aware of what is going on all around when the body is supine and brain activity is reduced to a minimum.[1] True, such evidence is largely anecdotal but no more, perhaps, than many medical case histories. Similarly, while both authors touch on that vexed and still unsolved question of whether there can be any mental activity after the dissolution of the brain at death, they stop short of paying tribute to those who, against enormous odds and in the face of ridicule, have made a serious attempt to obtain some empirical evidence on this point.

Again, Eccles shows himself fully alive to the potential relevance of our ability to manipulate mental images to the control we exercise over our own bodily movements (p. 286). But what if we could use imagery to gain control over objects in the external environment? What if we could imprint images direct onto a sensitive film? Or, by mental effort alone, cause a metal object to assume some imagined shape? To ask these questions is not to indulge in idle fantasy but to draw attention to reports that such effects do perhaps occur.[2] Of course, no one is asking Professor Eccles to accept these claims without further ado but those who are struggling to validate them would appreciate a few words of encouragement.

---

[1] A brief popular account of a number of such 'out-of-body' experiences reported by patients who 'in the course of accidents or severe injury or illness came very close to physical death' or who had been 'thought, adjudged or pronounced clinically dead by their doctors' has been published by a physician, Dr R. A. Moody M.D. [1975]. He has a file on some 150 such cases but suspects that they are much more common than would be supposed.

Thus, parapsychology would appear to complement any strong dualist view of the mind-body relationship. Why, then, do our two authors hold themselves aloof from it? For, in this instance, it would be absurd to suppose that they did so for fear of compromising their academic respectability. In their joint preface they announce: 'we have decided not to refer to parapsychology, of which neither of us have had any direct experience'. But, obviously, with two such polymaths, there must be more to it than just a question of ignorance. Popper is somewhat more revealing when he informs us that, not only is he incompetent to discuss the topic but that: 'one can easily spend twenty years on the subject without becoming competent. This is due to the fact that the results—or the alleged results—are not reproducible nor claimed to be reproducible' (p. 117). Here, I think, we get closer to the heart of the matter for, inasmuch as parapsychology aspires to being an experimental science, its failure hitherto to achieve a truly repeatable experiment is its most serious weakness. But, is that sufficient reason to ignore it altogether? We would, after all, think poorly of a political scientist who ignored history on the grounds that historical events are not repeatable. Similarly, we have a right to expect a philosopher of mind to pay attention to reports that deal with certain spontaneous mental phenomena that have far-reaching implications.

I will end on a speculative note. While I hope that this book will succeed I am doubtful whether, given the present climate of opinion, it will make much headway. If, on the other hand, in the years ahead, parapsychological research *does* attain the take-off point, represented by the reproducible phenomenon, our whole intellectual climate will be so radically transformed that this book will be greeted as one of the seminal texts of a new era.

JOHN BELOFF
University of Edinburgh

REFERENCES

BLAKEMORE, C. [1977]: *Mechanics of Mind.* Cambridge: Cambridge University Press.

EISENBUD, J. [1977]: 'Paranormal Photography' in B. B. Wolman (*ed.*): *Handbook of Parapsychology.* New York: Van Nostrand Rheinhold Co., pp. 414–32.

HARDY, A. [1965]: *The Living Stream.* London: Collins.

HASTED, J. B. [1976]: 'An Experimental Study of the Validity of Metal Bending Phenomena', *Journal of the Society for Psychical Research*, **48**, pp. 365–84.

MAYR, E. [1963]: *Animal Species and Evolution.* Cambridge, Mass.: Harvard University Press.

MOODY, R. A. [1975]: *Life after Life.* Covington, Georgia: Mockingbird. (New York: Bantam Books 1976).

SACKS, O. W. [1973]: *Awakenings.* London: Duckworth (Harmondsworth: Penguin 1976).

---

[2] For a recent discussion of 'psychic photography' see Eisenbud [1977] and for an account of experiments on paranormal metal bending see Hasted [1976].

THE PREHISTORY OF CHANCE*

Ian Hacking's book is a most interesting attempt to solve the following historical problem. Gambling, based on chance mechanisms like astragali (anciently), dice, coins, *etc.*, has been a pretty entrenched occupation since the Babylonian Empire and before. Yet the development of a systematic theory of random phenomena did not, according to Hacking, commence before the second half of the seventeenth century. What was the reason? Laplace said that the theory of probability is good sense reduced to a calculus. If that is the case then it must seem that pre-1660 Europe was notably and oddly deficient in this faculty. Hence it would seem that either Laplace was wrong or that the necessary mathematical tools for the construction of the calculus were not yet developed. But if they were not yet developed, why not? After all, the history of science seems to indicate that the development of appropriate mathematical techniques often follows on the articulation of a scientific problem whose solution requires them. In that case, *pace* Laplace, the sorts of problem presupposed by the development of probability must have been absent before 1660. The fact that gambling is as old as the hills makes this conclusion very difficult to swallow, given that it seems not to have been a predominately low-caste activity (so that its patrons would be unlikely to have the education if not the ability to theorise about their hobby) and more especially in view of the fact that quite ancient dice have, as Hacking points out, apparently been constructed to give practically equal probabilities to their faces. This last fact would seem to argue that the connection of fair bets with long run frequencies was as old as good dice, which is very old and thus that the principal conceptual ingredients of modern probability theory have been there practically from the beginning. And yet, according to Hacking, systematic exercises in probability mathematics took place only after 1660—at least, in Europe. India, as Hacking notes, may well have performed them earlier. Also India at this period (about two millennia ago) differed from medieval Europe but was similar to renaissance Europe most notably in having a merchant class, encouraging the growth of actuarial proficiency, and a flexible system of numerals. A further feature of seventeenth century Europe was the existence in France and Italy of a privileged and wealthy caste cut off from political activity whose idleness was partly absorbed in war but also partly in assiduous gambling, and a growing class of professional mathematicians who could be induced to advise the nobility of the laws of this diversion.

It is hardly rash for the historian of science to hold up stimulus, in the form of a problem or problems requiring solution, and the existence of a conceptual notation allowing the development of appropriate analytical techniques, as necessary and sufficient for the appearance of the theory or theories in question. Just the reverse. The trouble is that if one is not too scrupulous in the phrasing of the 'explanation', it will be so elastic as almost to guarantee its consistency with the facts of the case. However, this sort of triviality can be avoided in our case by fixing the meaning of 'conceptual notation', for example, to include only algebra and the system of rational arithmetic. But then the explanation does *not*, surprisingly, appear to fit the facts. Both incentive and notation were there for a considerable time before 1660, as Hacking tells us: problems of appropriate wagers in games of chance were discussed, but nobody, he claims, caught on to a systematically successful method of attacking them, until 1660, when quite a lot of people did. Mathematicians of high calibre were not lacking either to employ on these problems: Galileo and Cardano both discussed them. Now first of all it seems to me not unreasonable in the face of these difficulties nonetheless to retain the explanatory scheme but to point out that some time must elapse before problems elicit successful solutions, even if all the ingredients are there, and that precisely when somebody hits on the right method of attack is itself a rather chancy affair. Secondly, notation *was* extremely cumbersome before the publication in 1637 of Descartes' *Geometry* which introduced modern, or nearly modern, flexible notation. For example, Cardano represented '$x^3+6x = 20$' by '*Cub. p*:6 *reb. aeq.* 20' (*Cubus plus 6 rebus* = 20); the solution he rendered

---

* Review of Ian Hacking [1975]: *The Emergence of Probability*. Cambridge University Press. £5.50. Pp. 209.

'℞V: *cu* ℞ 108 *p*: 10*m*: ℞V: *cu* ℞ 108 *m*: 10'

i.e., $$x = \sqrt[3]{\sqrt{108}+10} - \sqrt[3]{\sqrt{108}-10}.$$[1]

Finally, Hacking's dating of the birth of the mathematical theory of probability at around 1660 is controversial. F. N. David's opinion, for example, is that 'Cardano did . . . as far as we know, calculate the first probability by theoretical argument, and in doing so he is the real begettor of modern probability theory'[2]; and this was about 1526. Furthermore, Galileo, as Professor Hacking notes, gave the correct answer to why the probabilities of certain outcomes in throws of *n* dice differ from others; again in F. N. David's words, '[Galileo's] treatment of the problem is exactly that which we should use today and leaves us in no doubt that the calculation of a probability from the mathematical concept of the equi-probable sides of the die was clearly known to the sixteenth century mathematicians of Italy'.[3] If we are prepared to put back the birth of the theory from 1660 to the early sixteenth century, say, and ask the same question 'why then?' the answer becomes rather obvious: those first exercises in probability theory are just a part of the European scientific and mathematical renaissance, developing *pari passu* with the exploitation and augmentation of the legacy of Greek and Arab mathematics.

Thus Hacking's dating is rather crucial to the urgency of the problem. He does, at some length, in a chapter entitled 'The First Calculations', discuss the contribution of sixteenth century Italy, though he is not inclined to view these attempts as constituting the beginning of the modern theory:

> In none of its forms, and by none of its names, had the new probability entered the discourse of Europe until that time [*i.e.* 1660]. I do not in any way mean to play down the interest of that historical figure Cardano. Our study is not of great men but rather of an autonomous concept (p. 56).

The reference to an autonomous concept in this passage is highly significant, for Hacking's contention is that although people like Cardano might have been doing things that look like anticipations of contemporary intellectual activity, in fact they were not. They were not because they inhabited an alien intellectual environment over which as individuals they had little or no control. If we return to the point where Hacking considers particular cases of the stimulus+machinery explanation of the emergence of probability, we shall get the flavour of his own rather different views:

---

[1] Peter Urbach brought this example to my attention.

[2] David [1970], p. 13.

[3] *Ibid.*

> . . . no explanation of the sort I have been describing can explain very much. As the Indian example reminds us, these explanations although instructive are pretty nebulous. Perhaps the explanations and conjectures are directed at the wrong question. All take for granted that there existed an intellectual object—a concept of probability—which was not adequately thought about nor sufficiently subject to mathematical reflection. So, one asks, what technology was missing? What incentive was absent? These questions are appropriate only if the conceptual scheme of those earlier times had within it a concept of probability (pp. 8–9).

In other words 'technology' and 'incentive' cannot explain the advent of a concept. What can? The answer is that new concepts are generated from old by an apparently occult process which is compared to a mutation: our concept of probability, which seems to be identified by Hacking with a set of constraints determining a 'space of contemporary theories of probability', 'resulted from a transformation upon some quite different conceptual structure', according to him. The transformation is 'something like a mutation' of elements of that prior structure. And just as a mutation tends to retain vestigial traces of its former identity, so the premutant 'non probability has features that determined the peculiarities of our own concept'.

This 'strange model' of conceptual evolution is a sort of dynamical version of the static theories, themselves variants of Plato's, of Frege and lately Popper. All stress the autonomy of concepts (or Thoughts, or theories), but Hacking's derives an acknowledged Hegelian character from its thesis that contemporary 'conceptual space' is highly conditioned by previous ones. Furthermore those prior concepts which set such constraints on our own thought may well have been forgotten, engendering, according to Hacking, a rather neurotic tension. Thus he can explain the puzzlingly perennial diversity-in-unity of probability, manifested in its being simultaneously a theory of reasonable belief and of a parameter characteristic of a chance set-up. Despite strenuous efforts, from Cournot to Carnap, these aspects cannot be rent asunder because

> we are in the grip of darker powers than are admitted into the positivist ontology . . . we should perhaps imagine that concepts are less subject to our decisions than a positivist would think, and that they play out their lives in, as it were, a space of their own. If a concept is introduced by some striking mutation as is the case with probability, there may be some specific preconditions for the event that determine the possible future courses of development for the concept. All those who subsequently employ the concept use it within this matrix of possibilities (p. 15).

The 'striking mutation' is a mutation of the Renaissance concept of 'sign', as we shall see,

I have no doubt that much of conceptual evolution is accurately described by Professor Hacking, but speaking personally I should hope that these sorts of considerations have little or nothing to do with the evolution of scientific theories, and I take modern probability theory to be such. I shall indicate later on why I think there is an alternative to Hacking's conceptual prisons. He himself is a little less than wholehearted about what he calls his 'methodology':

> I do not ask the reader to swallow all this. The story told in what follows is of interest even if the methodology that led to it turns out to be silly (p. 16).

The play's the thing. The story as told by Hacking is undeniably of great interest. For long before the seventeenth century, he tells us, probability was a predicate of *opinio*, and indicative of the extent to which it was approved by the appropriate (usually ecclesiastical) authority. During the Renaissance there was a view prevalent among the practitioners of the 'low sciences' of alchemy and medicine that the sensible universe was a vast system of intelligible signs, of which a correct reading elicited the truth of things. We still speak of a change of colour of litmus paper as signifying the presence of, say, an acid. Such usage is metaphorical. But just generalise this practice to regarding any empirical condition A entailing a state B as literally being in the relation of sign to thing signified and we have, if I understand correctly, the view of things Professor Hacking seeks to draw our attention to as typical of one element of Renaissance thought. The author of the signs is, of course, God. Paracelsus, who is extensively quoted in this part of the book, writes of reading the firmament as of reading a letter 'that has been sent to us from a hundred miles off, and in which the writer's mind speaks to us.' And just as the authorship of a work, say, of doctrine, confers an appropriate degree of probability on that doctrine, so adducing the testimony of the Ultimate Authority in His natural signs confers probability upon a conjecture about His world. But the signs do not uniquely determine in general, and *how* probable a conjecture is, given the sign, appears to have been estimated by the frequency with which that sign has previously signified the trait or thing cited in the conjecture as being nominated by the sign. Two interesting conclusions emerge from this analysis. First, in this view of the world the distinction between external (testimonial) and internal (empirical) evidence is obliterated; indeed, the distinction seems to have arisen only when experiment had become intellectually divorced from its previous testimonial context. Second,

> it is here that we find the old notion of probability as testimony conjoined with that of frequency. It is here that stable and law-like regularities become both observable and worthy of observation. They are part of the technique of reading the true world (p. 43).

With the eighteenth century substitution of 'empirical conjunction' for 'reason' in the analysis of 'cause', *opinio*, the meed previously of just the low sciences, displaces demonstrative *scientia*, and induction can take central place in the methodology of the sciences (disciples of Popper will not like learning that his methodology is naturally embedded in the older demonstrative view of knowledge).

The provenance of 'the problem of induction' and its connection with probability are thus explained. In summary then, Hacking's account seems to be as follows (I say 'seems' because I did not find this book easy to understand

and I suspect others will experience difficulty—even after several readings, and it is possible that I have not correctly grasped Professor Hacking's points): probability as we know it emerged in the 1660s in the sense that it only became 'public property' then. Before then it was not around in the intellectual environment; and it was not around because it presupposed the development of the notion of internal or empirical evidence and the connection of this with frequencies. It has an apparently inseparably dual nature because of the evolution of the concept of internal evidence out of the old theory of natural signs, in which frequency was a gauge of veracity.

Hacking's theory tells us we should date the emergence of probability at the date he points to; while we have seen that other authorities are inclined to select a much earlier date. Their justification, we have seen, goes something like this: in the sixteenth century people like Cardano and Galileo performed some calculations in combinatorial probability. Now these are the simplest sort of calculations being based on the assumption of finite sample spaces with equiprobable elementary outcomes, but they were the staple exercises until deviations from equiprobability could be systematically dealt with by the development of a theory of estimation, the first milestone on the way to which was Bernoulli's theorem. It is notable that the history of probability from the publication of Bernoulli's theorem until the end of the century is almost exclusively concerned with the discovery of better interval estimates of a binomial parameter (it was Laplace's great achievement in this field to complete de Moivre's work by evaluating the closeness of the approximation of the binomial to the limiting normal). Even Bayes's memoir, celebrated for its construction of precise inductive probabilities, seems to have been intended as a solution to the estimation problem.

So we have two different accounts of the emergence of probability. One dates the beginning of an epoch at 1660 and writes off the sixteenth century as insignificant from the point of view of the development of the theory; the other sees in those efforts the inauguration of the modern theory, and part and parcel of the European mathematical and scientific renaissance. A fact which seems to me to tell in favour of the latter (though this is highly tentative, for Hacking knows a lot more about the history of probability than I do) is that on the account proposed in this book one would expect right at the beginning a theory based explicitly on the measurement of probabilities by frequencies, rather than on the combinatorial probabilities which the mathematicians, like Pascal and Huyghens and even Bernoulli seem almost exclusively to have exploited. No doubt stable frequency differences between events, especially rather obvious ones, were felt to indicate probability differences, and the quotations adduced by Hacking are quite convincing on this point. But this does not make the early theory a frequency theory, *inter alia.* It is certainly a conceptual economy to invoke frequencies as measures *derivatively*, *via* the limit theorems, rather than independently postulating them, and as I suggested earlier, the marked eighteenth century interest in refining frequency estimates acknowledged Bernoulli's limit theorem as its stimulus. What in other words I am conjecturing is that the interpretation of probability as a frequency—or approximately a frequency—in long sequences of independent trials with constant probability (for not any frequency will do) arose as a result of Bernoulli's theorem: conceptual economy was actually observed. If I may mimic Hacking's disavowal: I may be wrong, but it's a nicer story.

If this quite 'rational' reconstruction is actually what happened, then the importance of the conceptual environment in the seventeenth century, or indeed at any other time, for understanding the development of the theory is rather limited. For the features of the theory which most claim our attention, like its frequency 'interpretation', for example, can be easily explained, it seems to me, as consequences of a theoretical structure which came to light with its deductive articulation. Doubtless *some* informal notion was enlisted in the first postulations which we call the initial phase of any theory, but as is often the case, these are compact of more deductive consequences than are apparent at the time. As these are revealed, often quite striking properties emerge, and nowhere I think is this more true than in the case of probability theory. The case of what Hacking calls 'epistemic probabilities' is in point here, for they emerge out of the probability calculus in two quite distinct ways during the evolution of the theory. First, Bayes in the famous memoir I have mentioned derives a posterior distribution for a probability parameter in an apparently quite rigorous way (his mathematical argument seems to be faultless). This is remarkable enough, but it is then an easy matter, as Bayes himself showed, to eliminate all reference to what Hacking calls the 'aleatory' parameter, and to compute the purely 'epistemic' probability of an event conditional on it having occurred in n previous trials with a given frequency. At this point probability bifurcated into its epistemic and 'aleatory' parts, in both of which considerable mathematical labour was subsequently invested. Each will in general evaluate an event differently, though each is an offspring of the same theory. Second, Boole remarked in his *Laws of Thought* that the algebra of events is the same as that of truth-values (cardinalities apart) and that mathematically probability theory is just the theory of a measure (as one would now say) on such an algebra. Now probabilities could be defined in a formally satisfactory way on sentences without reference at all to stochastic trials. What are these probabilities? Degrees of reasonable belief are good candidates. Carnap and the λ-continuum become possible, and the great systems of inductive logic of this century (not that they have been all that satisfactory in other ways). And all this comes out of a theory (to take Huyghen's presentation) of the fair price for a lottery ticket. It seems we are saved from Hegel after all. The inability, or at least reluctance, to divest one or other of the 'aleatory' and 'epistemic' notions of the title 'probability' (Cournot suggested 'chance' for the former, Carnap 'probability$_2$') is on this view to be explained not by reference to those 'darker powers' favoured by Hacking, but by reference to theoretical consanguinity, a common descent from the ancestral probability calculus.

Be all this as it may, Hacking's story is nevertheless an absorbing and informative one. He takes the reader on a fascinating intellectual journey through the scientific/philosophical background to the Renaissance, and on to the eighteenth century, pausing for more extended surveys of characteristic local features: Pascal's wager, the Roannez circle in Paris, the Port Royal Logic, Bernoulli's work in probability, Leibniz's designs for an inductive logic; and a great deal more. For a shortish book, it contains an enormous amount of information and comment. I found some of it difficult to grasp (I still do), but I suspect that this is due to a great extent to the considerable compression necessary to cram all this material into 200-odd pages. The consequent concision is unfortunate because it tends to engender obscurity and may even cause the reader, as it caused me, to wonder even after several readings of crucial passages whether the author's intention has really been grasped. Also, I doubt whether the author's rather jocular attitude to his principal theses will help sustain the less tenacious reader through these difficulties of exegesis.

To what extent, to conclude, does the research exhibited in this book contribute to 'liberating us from the cycle of probability theories that has trapped us for so long'? There is certainly something a little perplexing about the fact that the debate about what probability is and is not seems to go on and on. I don't myself feel that it is simply a matter of 'understanding our space [of theories of probability] and its preconditions' in order to be liberated from this controversy: I have tried to indicate why I think this sort of conceptual archaeology is unlikely to be helpful. (The analogy with a neurotic supposedly healed by being brought face to face with a past he has repressed, which seems to be what is suggested on page 16, is surely a bit of tongue-in-cheek). Much of the contemporary debate, it seems to me, is unnecessarily prolonged and controversial because of ingrained philosophical prejudice, usually of a positivistic kind: de Finetti's refusal to countenance a physical interpretation of probability is a case in point. Once the vestiges of extreme Positivism have made their departure, together with facile equations like 'probability = long run relative frequency' or 'probability = reasonable betting quotient', I predict a more settled future for the subject. The sorts of philosophical intolerance we have seen ought, however, to find a strong antidote in Professor Hacking's sympathetic book.

COLIN HOWSON
London School of Economics

REFERENCE

David, F. N. [1970]: 'Dicing and Gaming' in E. S, Pearson and M. G. Kendall (*eds*): *Studies in the History of Statistics and Probability*, pp. 1–19.

REDUCTION IN PHYSICS*

I have willingly agreed to the proposal to review Yoshida's monograph because the author deals with the same problems as I do in my [1977] English book which appeared at the same time and our aim is very similar: to defend the rationality of science.

Yoshida says in the preface that he was struck as an undergraduate by the 'replacement view of reduction' as being wrong (it would be better to say 'replacement view of scientific change' because adherents of this view usually do not speak about reduction). He argues that there is 'deduction in reduction'. When we have a more comprehensive theory T and a less comprehensive one T' there is always a possibility to deduce T' from T plus some auxiliary assumptions A_i. This is denied by P. K. Feyerabend, T. S. Kuhn and some other con-

---

*Review of Yoshida, R. M. [1977]: *Reduction in the Physical Sciences*, Philosophy in Canada: Monograph Series 4. Halifax, Nova Scotia. Pp. 90.

temporary philosophers of science. The monograph is to a great extent devoted to refutation of their arguments against the 'deductive view'.

The main argument used by Kuhn and Feyerabend is from ***meaning variance***. They say the meaning of terms changes in the course of transition from one theory to another; the new theory deals with other concepts, the old and the new theories are ***incommensurable***, hence any deduction is impossible. A favourite example of the 'incommensurabilists' is 'mass'. They say this term has another meaning in Einstein's Special Theory of Relativity (*STR*) than in Newton's Classical Mechanics (*CM*) because 'Einsteinian' mass is not constant, it depends upon velocity, it is connected with the energy according to the equation $E = mc^2$ *etc.* Yoshida answers that if all these dependencies were a part of the meaning, the relevant laws would be self-referring. *E.g.*, the law $E = mc^2$ would say that 'the product of the measure of that inertial property of a body which satisfies $E = mc^2$, amongst other things, and the square of the velocity of light (which velocity satisfies $E = mc^2$, amongst other things) is equal to the energy (which energy satisfies $E = mc^2$, amongst other things).' In short, 'those properties that satisfy me satisfy me' (p. 10). In other words, laws of physics would be necessarily true, analytic and known *a priori*. However, they are not. When physicists pass from *CM* to *STR* they do not give a new definition of 'mass', they speak as hitherto about the property 'in virtue of which a body resists a change in motion'; simply, the *STR* denies some claims of *CM*, *e.g.* 'the mass is constant' (see: *Appendix*: On the referent of 'Mass', pp. 80–4).

Yoshida observes that the term 'concept' is ambiguous. Logicians usually define 'concept' as 'meaning of the term'. However, when someone writes about the history of the concepts of space, force, atom, *etc.*, he often does not mean changes in meaning of terms but new theses, new synthetic statements, new 'theoretical identities', which are known *a posteriori*. When someone says that Einstein changed our concept of space, Maxwell changed our concept of light, Darwin changed our concept of species, he means that they changed our beliefs about the nature of space, light, species, but not meanings of words. The 'concept' is here much more than the meaning of a term. When somebody does not see this, he makes a 'fallacy of equivocation' connected with the 'fallacy of meaning variance' (pp. 17–20).

I fully agree with all this. Now I pass to a problem where my agreement is only partial—the problem of ***incompatibility***. Kuhn and Feyerabend say that a new theory is incommensurable with the old one. Their adversaries often say that such theories are commensurable but incompatible. The incompatibility is also an obstacle to deduction. For this reason, many philosophers stress now that we cannot, strictly speaking, deduce the old theory from the new one (and additional assumptions), we can only deduce a theory which is similar to the old one, but not identical with it. *E.g.*, Galileo's free fall law assumes that g is constant; however, Newton's gravitation law shows that g grows during the falling (because the distance to the Earth centre decreases). Hence these laws are incompatible and the deduction is impossible.

Can we surmount this obstacle and save the deduction? I believe (with Yoshida) we can. In order to deduce Galileo's law from Newton's *CM*, we must make an idealising (*i.e.* false) assumption that Earth's radius is infinite. We must also make other idealising assumptions: there is no air resistance, other bodies except Earth do not attract the falling body, no other (non-gravitational) forces act on it, *etc.* Only then we can deduce the free fall law from *CM*. We do not obtain a 'similar' law but a new interpretation of the free fall law: it is strictly valid only under all these idealising conditions. In general, the conjunction of a new (more comprehensive) law and some idealising assumptions entails the old (less comprehensive) law, though not in its original interpretation but in a new interpretation (in that the idealising assumptions necessary for the validity of the law are revealed). There are some other logical difficulties which I discuss in my aforementioned book.

Yoshida goes in the same direction saying that we must make false assumptions (he does not use the term 'idealisation'). He rightly stresses that we cannot state true auxiliary assumptions, *e.g.* in the case of the free fall law we never can take into account all forces which act on the falling body. However, he sees no problem here. He says the more comprehensive theory and the less comprehensive one are incompatible only when we make true auxiliary assumptions but we never make them (pp. 3–4). He is right when he insists that science cannot develop without false assumptions (I prefer to say: idealisations). However, he neglects logical difficulties connected with this.

The author notices that the term 'to reduce' is used in different ways. Sometimes physicists say that, under certain conditions, Newton's gravitation theory *reduces* to Galileo's law, or quantum mechanics (*QM*) to *CM*, *i.e.* a more comprehensive theory to a less comprehensive one. On the other hand, they say thermodynamics ***is reduced*** to statistical mechanics, or physical optics to electromagnetic theory, *i.e.* the less comprehensive theory to a more comprehensive (pp. 64–5). I think only the second expression is correct. Yoshida accepts both expressions pointing out the difference between 'reduces' and 'is reduced'. True, he proposes different names for the two relations: in the first case 'theoretical transformation', in the second case 'reduction'. I do not see a reason for this differentiation: in both cases we have a deduction (Yoshida also stresses this). We may always say that a (less comprehensive) theory T' is reduced to a (more comprehensive) theory T when the conjunction of T and some auxiliary assumptions A_i entails T'. *E.g.*, Galileo's law is reduced to Newton's theory. The expression 'Newton's theory reduces to Galileo's law' causes only confusion.

The main problem is the problem of falsity of A_i. The author says they may be false but they 'must be approximately true though strictly speaking false' (p. 71). I agree though it would be better to say that an idealisational law is approximately true in such real systems in which the idealising assumptions are approximately true. *E.g.*, there are no free falling bodies but there are conditions in which the fall is approximately free. If there were no such conditions, the idealisational law would be useless. However, I cannot agree with the author's thesis: 'An auxiliary assumption may be logically incompatible with a non-essential statement of the more comprehensive theory but it cannot be incompatible with an essential statement of more comprehensive theory' (p. 74). However, *CM* in the light of *STR* is true only under the idealising condition $c \to \infty$ which is incompatible with one of the very essential theses of the *STR*, $c = const.$ Similarly, *CM* is true in the light of *QM* only under idealising condition $h = 0$, but the thesis $h > 0$ is essential for *QM*. Everywhere we have a contradiction which is a serious obstacle to the reduction. Can we surmount this obstacle? I think we can, but not in such a simple way as Yoshida tries to do it. I suggest for this aim a procedure which I call 'abstracticization' (see my book).

Besides 'reduction' and 'theoretical transformation', the author speaks about a third intertheoretical relation introducing a neologism 'explimination'. It is an explanation by elimination of an entity; *e.g.*, the explanation of combustion by Lavoisier by elimination of phlogiston. Yoshida claims that there is also a deduction here. We may deduce the phlogiston theory from the oxygen theory replacing phlogiston by oxygen with the sign 'minus' in the chemical equations. Similarly, we may deduce the caloric theory of heat from the kinetic theory. In general, 'explimination' is a deduction of a 'defective theory T' from the conjunction of T and A_t in order to provide a basis for eliminating . . . an element from our ontology' (p. 78). I prefer to speak about replacement here. A correspondence relation (a deduction with need of a reinterpretation of the old theory) holds only between 'non-defective' theories of the mature science.

Yoshida's monograph contains an analysis of many examples of reduction in physics. Nevertheless, the author does not deal with the problems of the growth of science, he analyses only logical relations among theories. He always uses expressions 'more comprehensive' and 'less comprehensive', never 'earlier' and 'later' or 'succeeding'. He does not speak about correspondence relation or correspondence principle (although he analyses 'the logic of the limiting case'). Of course, this is not an objection, only a statement of facts. However, Yoshida sometimes makes remarks about the growth of science. He notes at the end of the monograph that 'the cumulative view of scientific change has good support (contrary to Kuhn)' (p. 79).

In my opinion the situation is more complex. Both one-sided cumulativism and one-sided anti-cumulativism are false. There are revolutions in science. These revolutions (in the mature science) are two-sided: they have a cumulative aspect (correspondence relation, deduction) and an anti-cumulative, revolutionary aspect (refutation of some old theses, reinterpretation of old theories in the light of new ones). This 'dialectical' view is presented in my book.

I think, however, that in our time, when relativist and anarchist views are widespread, the main task is their refutation, the stress on the cumulative aspect of science. For this reason I welcome Yoshida's book.

WŁADYSŁAW KRAJEWSKI
University of Warsaw

REFERENCE

KRAJEWSKI, W. [1977]: *Correspondence Principle and Growth of Science.* Dordrecht: D. Reidel.

Reviews

KRAJEWSKI, W. [1977]: *Correspondence Principle and Growth of Science.* Dordrecht: Reidel. \$19·50 Pp. xiv+138.

I cannot recommend this book though some aspects of it are commendable. Written by a Polish philosopher of science it gives us a few glimpses of philosophy of science in Poland and Russia *via* one or two paragraph summaries of the work of various philosophers. He does a good job of whetting our appetites for the likes of Amsterdamski, Nowak, Kuznietsov, Such, and so on—many of whom are also published by D. Reidel. I hope more of this is done. However this is not the main purpose of the work.

The main thrust is the analysis of the correspondence principle and a description of its role in the growth of scientific knowledge. And it is here that the book fails. It fails primarily from thin argumentation and an ill-conceived analysis of laws.

Consider Krajewski's account of the correspondence relation: a law L_1 is in a correspondence relation with a law L_2 if, and only if, (*i*) L_1 is formulated before L_2, and (*ii*) the equation $F_1(x) = 0$ (his shorthand for the familiar equations, *e.g.* $F = ma$ is conceived of as $F - ma = 0$) of L_1 is derivable from the equation $F_2(x) = 0$ of L_2 and the assumption that some parameter p_i which is characteristic of L_2 equals zero (p. 42) (he has the assumption that the parameter takes on the limiting value of zero in mind). Straightway he has a problem, for the assumptions that are made in establishing the correspondence relation between the special theory of relativity (hereafter STR) and the general theory (hereafter GTR) are that the diagonal components of the metric tensor are equal to plus or minus one (as well as that the non-diagonal components are zero). He cannot wave this case away as he does the coefficient of light absorption of a perfect black body (p. 29). This counterexample shows that this part of his account is defective. Moreover it is not clear why he should want to hold such a view in the first place.

He wants to claim (as do others) that Newton's laws of motion (hereafter CM) stand in a correspondence relation with the equations of motion of STR, and the laws of dynamics of quantum theory, but he sees two problems: (1) often, if not always, L_1 is incompatible with L_2, for example *STR* is incompatible with *CM*. 'How then may we say that we infer the laws of CM from the laws of STR and some limit conditions?' (p. 11). (2) The limiting parameter never takes on the extreme value—it is physically impossible. Thus the assumption of that value is false and since 'we know from elementary logic that "the falsity entails everything" . . . we perform a kind of reasoning forbidden by logic' (p. 10).

I do not find these 'problems' to be much by way of problems, for elementary logic also says that one can infer a false statement from a conjunction of a true and a false statement. Thus even if we assume the truth of L_2 and the falsity of L_1, the alleged incompatibility of L_1 and L_2 creates no logical barrier to the derivation of L_1 from L_2 and the limiting condition so long as the limiting condition is false. This takes care of the first 'problem'. The second 'problem' implies at best that the limiting assumption *is* false, and that raises no difficulty as we have just seen. Perhaps what bothers Krajewski is that the limiting assumption may contradict an implicit supposition of L_2. The derivation would then seem trivial and uninformative since it would be based on the principle that a contradiction implies everything. (For some reason unknown to me he believes such inferences are forbidden by logic.) But the solution to that seems obvious: one need insist only that use must not be made of the contradictory (or contrary) of the limiting assumption. This seems reasonable since scientists do not make use of the principle that a contradiction implies everything.

In any case Krajewski's solutions to the 'problems' entail a fair amount of machinery. He claims that scientists usually consider $F(x) = 0$ to be the law. However, the conditions under which it is alleged that the equation is true, $C(x)$, are an integral part of the law. (The latter cries out for support but none is to be found.) As a result we get the familiar positivistic analysis of a law as a universal empirical generalisation of the form $(x)(C(x) \rightarrow F(x) = 0)$. But he adds a new twist allegedly to put his views in accord with the ordinary use of scientists: 'we shall say that when F changes we have a new law and when C changes we have a new expression (formulation) of the same law' (p. 14).

I think it is quite clear that this will not do if only because it implies that one and the same law and therefore statement may be both true and false simultaneously. The classical law of addition of velocities, 'under all circumstances, $v = v_1 + v_2$' (he gives no antecedent (see p. 49) so I am guessing here, but without one it would be a counter-example to his analysis) and '$c = \infty \rightarrow v = v_1 + v_2$' are on his view, different expression of the same law. It is quite clear that this is absurd. From the standpoint of *STR* the first statement is false and the second true. They cannot therefore be different expressions of the same law.

An alternative is to deny that 'law' denotes one statement as did N. R. Hanson in *Patterns of Discovery*. Here one could argue either that 'law' denotes a set of statements some of which may be true, some false or that 'law' is referentially ambiguous. But these are views which I find neither plausible nor in this book. Sets are neither true nor false, laws are; and the question 'To which of Newton's second laws are you referring?' seems to be self answering.

Curiously the author uses the device of simultaneous truth and falsity to 'solve' the problem of incompatibility (the first 'problem'). Essentially his solution is that '$c = \infty \rightarrow v = v_1 + v_2$' is true if *STR* is true (since it is derivable), that law *is* the classical law of addition of velocities, *ergo* the classical law is true if *STR* is true. The two laws—the familiar relativistic law, 'in all circumstances, $v = v_1 + v_2/(1 + (v_1 v_2/c^2))$' and the classical law—are not therefore incompatible, and the supposed barrier to derivation is lifted. The classical law, 'in all circumstances, $v = v_1 + v_2$' is false; '$c = \infty \rightarrow v_1 + v_2$' is true (if *STR* is true); both sentences express the classical law because of identical consequents; then the classical law is both true and false—which is absurd.

Krajewski's solution to the second 'problem' does not fare any better. The problem: '$[v = v_1 + v_2/(1 + (v_1 v_2/c^2))]$ supposes the assumption $c =$ const.—the basic assumption of the whole *STR*. On the contrary $[v = v_1 + v_2]$ presupposes the assumption $c = \infty$ which is incompatible with the former. Therefore, we cannot deduce the law of *CM* (in its contemporary interpretation . . .) from the law of *STR*' (p. 49). It is unclear to me how the relativistic statement supposes $c =$ const. or how the classical statement supposes $c = \infty$ for however I take them the claims seem to be false. It is quite possible for the relativistic statement to be true and for $c \neq$ const. Moreover it is possible for one to know that the relativistic statement is true without knowing that $c =$ const. And if the classical statement presupposed that $c = \infty$, the nineteenth century measurements of c would have shown *CM* to be false long before it has ever been thought to have been shown defective. One suspects the author is committed to the theory-determines-the meaning view more than he is aware (*cf.* p. 56). Worse is the fact that '$c =$ const.' and '$c = \infty$' are not incompatible as he claims (light might be propagated instantaneously whenever propagated). Still worse is the fact that even if they *were* incompatible, it would not follow that the one law would not be derivable from the other. Indeed one would be derivable just because of the incompatibility. He must mean it is not possible to derive one from the other without making use of the incompatibility, but even this does not follow and is moreover false. As he points out later, one can derive the classical equation from the relativistic one plus the assumption $c = \infty$ (p. 49).

His solution: abstracticisation. One begins with the law of *STR*, '$c = \text{const.} \rightarrow v = v_1+v_2/(1+(v_1v_2/c^2))$', then one 'abrogates' the basic assumption to obtain the consequent. To this 'abstracticised' law we add the idealising condition $c = \infty$ and derive '$v = v_1+v_2$'. Presumably we use conditionalisation and obtain '$c = \infty \rightarrow v = v_1+v_2$' which is, by the author's criterion of identity, the classical law.

Like the solution to the first problem this solution comes to grief on the identity of a law, '$c = \infty \rightarrow v = v_1+v_2$' is not the law of our classical fathers. Nor should '$v = v_1+v_2/(1+(v_1v_2/c^2))$' be the law of *STR* on his view for it does not have an antecedent. And on his view if we add an antecedent other than '$c =$ const.' we get something which would be false while the relativistic law is supposedly true. Thus the derivation is neither a derivation *from* STR nor *of* the classical law, and therefore neither is his solution a solution.

The above are examples of the thin argumentation to be found in this work. Other examples are to be found in the author's discussion of the question of meaning variance.

Meaning variance arises as a third problem confronting his version of the correspondence principle. Meaning variance theorists would argue that he may have established that the same or similar law *sentence* can and should be derived from the law *sentence* of the successor theory, but he has not shown and cannot show that the same *statement* is so derivable because the meaning of the constituent words will have changed in the transition from the one theory to the other. A consequence often drawn from the latter is that the two laws and/or theories will, therefore, be incommensurable.

The author rehearses some of the standard criticisms, claims that they are insufficient without argument, and proceeds to provide some others. Some examples:

> (*a*) *CM* defines mass as a measure of inertia . . .; *STR* does the same . . . . Hence the meaning of the term 'mass' is the same [in both] (p. 56).

The conclusion is supposed to follow by a rather simplistic theory of meaning: 'The meaning of a word is determined by its definition' (p. 56). Meaning variance theorists would agree that 'mass' means ***at least*** that to be sure, they would not agree that it means *only that*, and it is the latter which is supposed to give them their variance claim.

> (*b*) $CM\colon\ m(x) = \text{const.}$
> $STR\colon m(x) = f[v(x)] \neq \text{const.}$
>
> The statements . . . are incompatible but they use the same concepts (otherwise they would not be incompatible) (p. 57).

The claim of incommensurability which is supposed to follow from meaning variance just is the claim that statements such as the above are not incompatible. Thus to argue that they are incompatible therefore there is no meaning variance is to beg the question. A similar type of argument is used again on pages 59 and 60.

> (*c*) The Kuhnian thesis, that the 'Newtonian' mass is constant and the 'Einsteinian' one is variable is misleading. There are not two concepts of mass. There is only one concept of (complete) mass. Newton and Einstein have formulated incompatible statements about it. Newton's statement . . . is false, Einstein's . . . is true. The situation seems to be trivial. (p. 57)

Most philosophers are convinced that there are two concepts of mass because it is a necessary truth that Newtonian mass is conserved (at least for fundamental bodies or corpuscles) because Newtonian mass just *is* that mass which satisfies the laws of Newton. And it is a necessary truth that Einsteinian mass is variable because Einsteinian mass just *is* that mass which satisfies the laws of STR. The important question which the author neither asks nor answers is whether 'mass' as it occurs in those theories refers to either one of these concepts.

Given some sort of understanding of the correspondence principle the question arises as to its range of validity. According to Krajewski the correspondence principle is valid only for a mature period of a science's history. But this thesis seems as unsatisfactory as his other claims. Consider the Wien radiation law for the equilibrium distribution of energy in a cavity at temperature T, $u_\lambda = (C_1/\lambda^5)e^{-C_2/\lambda T}$, and the Rayleigh–Jeans law, $u_\lambda = 8\pi kT/\lambda^4$. These are bits of mature science, the latter was formulated after the former, yet it is doubtful that one can derive the former from the latter under Krajewski's conditions. Thus either the principle is false or the claimed range of validity is false.

Besides the weaknesses noted above, the book has many irritating inconsistencies, *e.g.* 'The ideal law is fulfilled only in the ideal models, hence it is not directly testable' (p. 23), and then '. . . in many cases we may apply and test directly idealizational laws . . .' (p. 28); 'Statements treated as candidates for laws . . . are always strictly universal ones, *i.e.* they must not contain proper names, . . .' (p. 13), and then [Kepler's laws] often cause trouble for philosophers of science because they contain a proper name, "sun" (p. 49 n.); and 'reduction is a . . . deductive-nomological explanation . . .' (p. 30), then '[w]e do not say that L_i is explained by L_f though there is an obvious strict reduction here . . .' (p. 39).

Finally there are many faults that must be laid at the door of the editor. There are idiomatic mistakes, for example, 'limit case' instead of 'limiting case' (pp. 1, 6); '(Quoted after Kuznietsov, 1948)' instead of '(Quoted from Kuznietsov, 1948)' (p. 7); 'Galileo's free fall law describes free falling' instead of 'Galileo's law describes free fall' (p. 26); 'Newton law' instead of 'Newtonian law' (p. 27); and 'Many authors abandon the implication at all' instead of 'Many authors abandon the implication completely' (p. 45). There are unnecessary neologisms or unfamiliar terms, for example 'explanative' rather than 'explanatory' (p. 45); 'precisioning' rather than 'clarification' (p. 46), 'abstracticization' rather than 'abstraction', 'abstracticized' instead of 'abstracted' (p. 49); 'of testification' rather than 'for testing' (p. 62); 'needs precisioning' instead of 'needs to be made precise' (p. 95); 'synthetical' instead of 'synthetic' (p. 104); 'hypotheticism' instead of 'hypothetico-deductivism' (p. 20); and 'mythic' rather than 'mythical' (p. 90). There are also misspellings, for example: 'zuberstaub' should be 'zauberstab' (p. 1); 'conformationism' should be 'confirmationism' (p. 70); 'Ruppert-Hall' should be 'Rupert Hall' (pp. 84, 133); 'Schiebe' should be 'Scheibe' (p. 89); and 'oxygene' should be 'oxygen' (p. 183).

RON YOSHIDA
University of Lethbridge

DAVIES, P. C. W. [1977]: *Space and Time in the Modern Universe.* Cambridge: Cambridge University Press. £6.50. Pp. 232.

Few contemporary philosophers would wish to maintain with Kant that the investigation of the structure of space and time is a purely philosophical matter that can be carried out in *a priori* fashion without the intrusion of empirical contamination. Doubtless any such investigation must involve both conceptual and empirical elements. In, for example, the Leibniz–Clarke Correspondence, Einstein's papers on relativity and contemporary discussions of Mach's Principle, one finds such an intimate mixture of the conceptual and the empirical that it is probably not possible and certainly not fruitful to attempt to delimit a range of issues that could be regarded as purely philosopical. So fully to pursue an interest in the structure of space and time one must become acquainted with the results, often highly technical, of recent work in physics. However, such is the fascination of questions concerning space and time that many students attracted to them lack any background in mathematics or physics. And if one is to convey to these students a sense of the relevance of contemporary physics, one needs to have available lucid surveys of recent work that are accessible to the educated layman. For this reason one can only laud the largely successful attempt of the author of this work to provide an up-to-date survey that should make exciting reading both for those with only the most rudimentary background in physics and for those whose acquaintance with the relevant physics is slightly dated. In this well-written book the qualitative presentations of various theories which are provided generally succeed in giving a sense of the theories without undue distortion. The intuitive content of sophisticated notions is lucidly given with the assistance of particularly perspicuous diagrams. In addition it is a largely non-partisan work in which no particular philosophical or physical axes are ground. Readers familiar with the issues either as philosophers or as physicists will find the discussion at times (for example, the characterisation of the relativistic-absolutist controversy) lacking in sophistication. However, it would be churlish to complain of this given the author's laudable intentions of producing an introductory survey. So in the chapter by chapter discussion that follows attention will be restricted to those points where the author's treatment is arguably misleading rather than simplistic.

In the first chapter, Davies introduces the notion of a mathematical model of space and nicely characterises in an intuitive way the crucial topological properties of mathematical spaces (*i.e.*, continuity, dimensionality, connectivity and orientability). Continuity is not handled as well as it might be. For the author suggests that the condition of infinite divisibility leads to continuity where in fact this condition does not give rise to Dedkind continuity but only to density. After introducing the notion of a metric space Davies turns to characterise the Newtonian conceptions of space and time and to contrast them with the relational conceptions. The familiar arguments that in the context of Newtonian mechanics the undetectability of motion with respect to absolute space counts in favour of the relativist theory and that the presence of inertia force effects seem to count in favour of an absolutist theory is competently developed. In considering the Machian counter to Newton, which Davies judges not to have been wholly successful, he draws attention to the fact that even within the confines of Newtonian mechanics inertia force effects can be accounted for in a more economical spacetime, neo-Newtonian spacetime, which allows for absolute acceleration without admitting of absolute motion. In fact the author talks of neo-Newtonian *space* where what he has in mind and what is required is neo-Newtonian *spacetime*. Neo-Newtonian spacetime is a matter of some significance that may not be familiar to many philosophers or physicists reading this work and it would have been helpful if a reference had been given to the relevant literature. A more advanced discussion is to be found in J. Earman's 'Who's Afraid of Absolute Space?' in the *Australasian Journal of Philosophy*, **48** (1970). It will not do to say as the author does that in a Newtonian world we would be compelled to reject Newtonian spacetime for Neo-Newtonian spacetime. Compulsion is not the correct notion here. For in the context being considered nothing would falsify the postulation of Newtonian spacetime in favour of neo-Newtonian spacetime. For in that context Newtonian and neo-Newtonian spacetime have the same observational consequences. It is simply that as Newtonian spacetime lacks the excess baggage of neo-Newtonian spacetime Occam's razor would incline us to favour the weaker of the two theories.

A lively and accessible account of the origins and content of the Special Theory of Relativity follows in chapter 2 with particular reference to the demise of the Newtonian conceptions. As so much of the controversy concerning absolute/relational conceptions turns on questions of meaning and intelligibility one could wish that these notions had been manipulated more carefully. For example, it will not do to say simply that the notion of uniform notion is without meaning (p. 31). Certainly the null result of the Michelson–Morley experiment counts against the postulation of a spacetime that allows for absolute motion. However, as we can imagine possible worlds and possible theories relative to which uniform motion would be detectable it cannot be said to be a meaningless notion. In the author's treatment of the Special Theory a mistake all too common in popular accounts of relativity is perpetuated in his claim that '. . . the theory of relativity demolishes the before-after relation between spatially separated events'. The truth of the matter is that the before–after relation remains an invariant under the Lorentz transformations for spatially separated events that are causally connectible by signals travelling at speeds equal to or less than that of light. In his detailed discussion of the 'twin's paradox' that follows the author accounts for the differential ageing by reference to the acceleration of one of the frames. It is, however, preferable to account for this by reference to the fact that the length of the spacetime path (which represents proper time) of the twin who remains on earth is longer than that of the spacetime path of his counterpart. For there are spacetimes (*i.e.* cylindrical spacetime) in which a version of the paradox can be generated for twins having twice intersecting *geodesics*. In this case one has to account for the asymmetry in terms of the differing lengths of their spacetime paths. Because of its greater generality this manner of accounting for the lack of symmetry ought to be preferred in the standard version of the twin's paradox.

Curved spacetime and the General Theory of Relativity are given an introduction treatment that could be digested with profit by someone with virtually no background in physics in the fourth chapter. There follows a very up-to-date discussion of black holes which is very much to be welcomed for avoiding the nonsense frequently written on this topic in popular accounts. In considering singularities reference might have been made to Hawkings recent work on the possibility that pair-production may occur in the context of rotating black holes which are not perfectly symmetrical. For many readers much of the material on black holes is likely to be new and hence the absence of any references to more advanced discussions is regrettable.

In his chapter (chapter 5) on modern cosmology, the author makes effective use of Olbers's paradox in introducing the evidence of the standardly accepted hypothesis of the expanding universe. Non-mathematical characterisations are then given of the standard pressure-free Friedman cosmological models with negative, zero and positive curvature. While it is the case, as the author claims, that these are the basic working models used by cosmologists it is most contentious to claim that the singularity which occurs in these models when the radius of the universe is zero represents the *creation* of the universe and of spacetime (and equally contenious to say that most cosmologists agree in this interpretation of the singularity). At the very least this interpretation should have been defended in some detail, especially in a work aimed in part at displaying the implications of the discoveries of modern physics for the beliefs we hold 'in everyday life' about space and time. No doubt one such everyday belief is that time had no beginning. In view of this one would like to have the reasons spelled out for taking the singularity as representing a first moment of time rather than representing the time of the first state of the universe before whose occurrence time extended unendingly back. Or, indeed, what are the grounds for thinking of singularity as representing a first state of the universe instead of representing merely the limits of our ability to extrapolate backwards in time? For as the author notes the General Theory breaks down at the singularity. Why then opt for the thesis that the singularity represents a first state rather than a state whose predecessors cannot be extrapolated with the aid of the theory used in the postulation of the singularity? In addition to this serious lacuna there is surprisingly little reference to the question of the sort of evidence which would be relevant to deciding between the competing Friedman models.

Chapter 3 ('The Asymmetry of Past and Future') discusses temporally asymmetrical processes (*i.e.*, processes the time reversal of which are not as a matter of fact observed to occur) with particular reference to the standard sort of thermodynamical process and to the apparently non-thermodynamical process of wave motion. It is argued that the occurrence of these processes without the occurrence of their temporally reversed counter-parts cannot be explained by reference to known physical laws as these laws are time symmetric. In chapter 6 ('The Beginning and the End') an attempt is made to explain this in the case of thermodynamical processes by reference to boundary conditions through the claim that the universe in expanding passed from a state of equilibrium to a state of disequilibrium. The author seeks to reduce the wave motion case to the thermodynamical case by outlining the Wheeler-Feynman thesis that the absence of advanced waves arises because the universe acts as an absorber. Under the assumption that the universe is approximated by a Friedman model, adopting this explanation of the non-occurrence of advanced waves requires rejecting the expanding Friedman models in favour of the contracting model. All in all this is one of the most unsatisfactory parts of the book. Little indication is given of the highly controversial character of the Wheeler–Feynman thesis. No other reasons are advanced for thinking that the universe will contract. And, more seriously, the reader is left entirely in the dark as to what possible explanation could be given of this asymmetry under the not entirely unreasonable assumption that the universe will continue to expand.

The final chapter begins with a diatribe against what the author sees as the collapse of confidence in science and the replacement of reason by superstition. One may well wonder if this regrettable interest in witchcraft, flying saucers and so on is as wide spread or as serious as the author takes it to be. It is apt to strike the reader as a case of 'the author doth protest too much' in view of his own speculations that follow about the possibilities of life beyond earth and the possibilities of communicating with such life. On page 210 we are told 'it certainly seems worth a modest effort to try and establish such communication in view of its enormous significance.' Surely, if that is to be taken seriously the possibility of extra-terrestrial visitors is to be taken seriously (not that either should be taken seriously). I am inclined to feel that this final chapter with its speculations about the conditions required for life (a cool, expanding isotropic universe is what is apparently needed), its critique of God and its metaphysical thesis about the relation of time and consciousness rather detracts from the book. It is a sort of concluding 'unscientific postscript' which at several points is inconsistent with what has gone before. For instance, the author speculates that the concepts of space and time may pass out of 'the interest and language of mankind'. While one can perhaps conceive that these concepts might cease to play a fundamental role in physical theories, it is hard to see how the author's more extreme claim can be consistently maintained in conjunction with the thesis advanced in the first chapter that time is the most elementary aspect of experience. And while one might readily agree that invoking an act of God to explain 'cosmological creation seems rather desperate', it will not do to claim that there is a straightforward logical contradiction in the thesis that creation was caused by a prior act of a God existing in time. The author sees this as incoherent only because he has adopted the thesis that it is incoherent to suppose time to have existed before the creation of the physical universe—a thesis for whose acceptance he gives us no reason.

Finally, many would be sympathetic to the author's view that in analysing the concepts of the past, the present and the future one has to look more to the realm of consciousness and language than to the realm of physics. However his particular claim concerning these notions is somewhat puzzling. For it is said that while physics can distinguish past and future the mind must be invoked in distinguishing between past, *present* and future. To see that this is puzzling let us assume with the author that physics licenses the use of an objective asymmetrical before–after relation. Presumably what is meant by the claim that physics can distinguish past and future is that in a given frame of reference at any moment of time, the past at that moment of time consists of all those events before that time, the future being all events after that time. But physics equally employs a notion of simultaneity within a given frame and the present at a moment of time (in the given frame) can be introduced as the set of all events simultaneous with that moment of time. Certainly the analysis of the notion of the notions of past, present and future is an open and controversial matter. It does seem clear is that these notions form an indissolvable triad, the content of each notion being given in part by reference to the others. What makes the author's remarks puzzling is his attempt to give a special mind-dependent status to 'is now' which is withheld from 'will be' and 'was'. If one member of this triad is in some sense mind-dependent, the others are as well. There are important and difficult issues here. Unfortunately the author's brief remarks are regrettably more likely to confuse than to enlighten. One would rather have seen his obvious skills as a lucid and lively expositor of scientific theories employed to students' greater advantage in a final chapter in the style of the earlier chapters. For there are topics whose coverage would have been useful. For example, the relevance of the vacuum solutions of the General Theory to the absolutist–relationalist controversy or the significance of the Gödel model with its closed world-lines that are everywhere timelike. Or, his unargued contenious and controversial contention that physics permits backwards causation (pp. 160, 183) might have been fruitfully developed.

It is important that there be books which disseminate the results of physical theories to the educated public at large. One can only hope that others will emulate the author's success in doing this. One feature that makes a book enjoyable reading is the absence of complex footnotes. However, the author in eschewing all footnotes, references and bibliography goes too far in the right direction. There are several points likely to be of interest to the specialist who will find his appetite whetted with no hints as to where further fare is to be found. And, most regrettably, the student will find no guide to further reading in the growing literature on space and time.

W. NEWTON-SMITH
Balliol College, Oxford

NARLIKAR, JAYANT [1977]: *The Structure of the Universe.* Oxford University Press. Hardback £4.25; paperback £1.95. Pp. 264.

Several general books on cosmology have been published recently, providing the potential reader with a wide choice of styles and points of view. This one is by Jayant Narlikar, Sir Fred Hoyle's student and colleague, who together have published many radical papers and one book about various aspects of the universe.

This book is one of the most attractive of the new crop. It is well and clearly written, at about the level of a first year undergraduate in physics. It is not as systematic as a textbook would be, but its generality is balanced by more detailed mathematical treatments of particular topics in 'boxes', with which the text is interspersed.

For the most part the treatment is straightforward and non-controversial, but the most valuable parts of the book are the occasional intrusions of the Hoyle–Narlikar points of view. It is made quite clear which these are, so that the reader can judge for himself how convincing a case has been made.

From the point of view of the philosophy of science, perhaps the greatest interest attaches to the chapter entitled 'The Universe and the Arrow of Time'. Here it is argued that there may be a connection between the arrows of time defined by the expansion of the Universe and by the second law of thermodynamics, particularly in connection with processes involving the emission and absorption of radiation. This question has been widely discussed recently, most comprehensively by Paul Davies in his book *The Physics of Time Asymmetry.* Some of the work of Hoyle and Narlikar has been devoted to this question, and it is good to have this general account of it.

One feature of this work is the provocative claim that an excited atom decays 'spontaneously' not as a result of its coupling with the quantum fluctuations of the vacuum electromagnetic field, but rather of its coupling with the rest of the matter in the Universe. On this view even distant matter would be important, because it has an advanced as well as a retarded effect on the atom. This variant of the well-known Wheeler–Feynman theory would undermine the whole of our understanding of quantum field theory. And therein lies my one quarrel with the author. Quantum field theory is one of the most thoroughly worked-out branches of physics, which is able to account for a wide variety of phenomena, especially in electrodynamics. In some cases agreement with observation is achieved to seven places of decimals, and no purely quantum electrodynamical phenomenon is known which shows any discrepancy with theory down to the smallest distances yet involved, namely 10^{-15} centimetres. Moreover this theory has been developed to be in harmony with quantum mechanics, whose wide agreement with observation is commonplace, and in particular with the Heisenberg uncertainty principle. Yet Narlikar's rejection of quantum electrodynamics is made virtually without comment.

What makes me most uneasy is that when he does refer to this theory he uses rather odd pejorative phrases. Thus, in referring to the properties which the theory ascribes to the vacuum, they are described first as 'unusual' and then as 'magical'. Yet these properties are dictated by the Heisenberg uncertainty principle itself, whose application to the electromagnetic field was first made by Dirac precisely 50 years ago! Of course, Narlikar in his theory is attempting to eliminate the dynamical aspects of the electromagnetic field altogether, and I applaud the attempt to do this to see how far one can get. But to stigmatise a quantum field as magical suggests a misunderstanding of the nature of physical theory.

This criticism involves only two or three pages of the book. The rest of it is splendid, and can be unreservedly recommended to anyone with modest physics who wants a general view of our present picture of the Universe.

D. W. SCIAMA
All Souls College, Oxford

LUCAS, J. R. [1976]: *A Treatise on Time and Space*. London: Methuen, University Paperback. £3.90. Pp. xi+321.

The welcome appearance of this book in a paperback edition is the occasion for this review. It is an intriguing, but puzzling work dealing with a wide range of the topics that fall within the philosophy of space and time, arguing its theses with care and insight, displaying a degree of learning impressive both in its breadth and depth, expressed in deft sentences of telling clarity. The puzzle lies in Lucas's methodology which shapes the whole book as a series of transcendental deductions of various fundamental properties of space and time. No doubt the proper study of mankind is man and philosophy should always be aware at least of where it stands with respect to this study. For Lucas, the aim is to show how much of what we find in the concepts of time and space derives from the human epistemic situation. This makes his book very interesting and a little mysterious.

Lucas begins with characteristically bold, provocative and penetrating claims.

> Time is more fundamental than space. Indeed, time is the most pervasive of all the categories. Some theologians say that God is outside time, but it cannot be true of any personal God that he is timeless, for a personal God is conscious, and time is a concomitant of consciousness. Time is not only the concomitant of consciousness, but the process of actualization and the dimension of change. The many different definitions of time given by philosophers reflect its many different connections with other fundamental categories. Time is connected with persons, both as sentient beings and as agents; it is connected with modality, and the passage from the open future to the unalterable past; it is connected with change, and therefore with the things that change and the space in which they change.

As the book unfolds we find, more and more, how much he thinks our epistemic requirements mould what we attribute to space and time as entities in or aspects of the real world. My remarks about the problem, as I see it, which Lucas's Kantian style of argument poses will lead me to comment on several among the wealth of topics which he deals with.

Perhaps the centrepiece of the book, its most ambitious and ingenious enterprise, is part II: 'The argument from time to space'. Lucas speaks of this as 'a "transcendental derivation" of philosopher's space from time'. Philosopher's space is 'a necessary concomitant of other fundamental categories—of time, of substance, of change, of motion'. The argument shows that 'if we have the concept of time, we must also have the concept of philosopher's space' which is argued to have a definite topology, some sort of metric and to justify 'the pre-eminence of Euclidean geometry'. (All these quotations occur on p. 97.)

The skeleton of the argument is this: a disjunctive necessary condition for there to be time is that some thing should change or that some conscious being should note time's passage.

Each disjunct in this necessary condition gives rise to a separate argument to the same conclusion: that we must have the concept of qualitatively identical but numerically distinct things. The left hand side of the 'benzene ring', as Lucas calls it (p. 100) tells us that only *things* can change, for feature-placing language is inadequate to capture the concept of change. Some things, like chemical substances, are such that if they are qualitatively identical then they are also numerically identical; these things cannot change so the concept of thing's changing requires the concept of qualitatively identical but numerically distinct things. The right hand side tells us that communication presupposes things as objective correlates of the message sent and that communication which is not otiose is contingent, hence about what might be other than it has been, which (weakly) implies change, things and so on. Further, communication uses tokens of types, which presupposes things qualitatively similar but numerically distinct.

Having reached this junction, the argument splits into two strands again. Lucas offers us a choice between a Leibnizian and a Newtonian option for understanding qualitatively identical but numerically distinct things. Each distinguishes between qualities and quasi-qualities. Qualitative identity is identity of qualities proper but numerical distinctness is difference in quasi-qualities. Leibniz distinguishes real from quasi-qualities only relative to some context of interests. Strictly, quasi-qualities are qualities. This gives us the Identity of Indiscernibles in a form applicable to angels and makes monads of our things. Newton offers a single absolute way in which things can differ while being in every real respect qualitatively identical. This respect must, in effect, be a parameter assigned to each individual to give it 'room . . . to be different in' (p. 123) and is philosopher's space.

One puzzle about this argument is that only the Newtonian option seems to yield the derivation of space from time. At this point, either I have lost sight of Lucas's argument or he has lost sight of his conclusion. In section 27 'Wireless Metaphysics', he goes on to show that in a pure sound world (described after the fashion of Strawson's sound world in *Individuals*) one can choose either programmes or stations as things. The former option is the Leibnizian one and Lucas shows, by introducing a kinaesthetic sensation as a counterpart of twirling the control knob, that we could find programmes in two 'places'. Perhaps so, but how does this show that the concepts of space is *required*? Leibniz certainly thought it dispensible and I do not see what reason Lucas gives us for rejecting, in this context, the ingenious scheme of the *Monadology*. Further, in the next section on Impenetrability, Lucas argues, plausibly enough, that spatial exclusion is a logical requirement of the Newtonian option. He goes on to argue as follows (p. 133).

> Each station is sandwiched in between two neighbours, and cannot get past them and be next to any other stations. And this is inevitable with impenetrable things located in a one-dimensional space. But it runs counter to our general, non-wireless, experience. Things not only can change, but can change in their relations to one another: they are not arranged in one invariant serial order. It follows that space, Philosopher's space, has more than one dimension.

But this argument surely depends heavily on 'our general, non-wireless, experience' and is no longer a transcendental deduction from our concept of time.

The argument is certainly ambitious, novel and perceptive enough to be worth a lengthy comment on its own account. But it provides a useful context in which we may see how crucially important and problematic Lucas's methods of argument are. The most frequently used tool in Lucas's transcendental methodology is the communication argument, exploited with much ingenuity in the derivation just sketched. But what do these argument prove and how do they prove it?

It would seem reasonable to assume that communication arguments begin from something like the premiss that all our concepts are moulded by the requirement on them that they must make communication possible. The conclusion, it would then seem, should take the form that our *concepts* have a certain structure, that we are bound to make a place in our scheme of concepts for those of a certain kind or that some concepts are required by others. Communication arguments would then have recognisable affinities with Kantian arguments. We should not be proving conclusions about the way the world is but rather about the structures which our thought about it has. That seems to be the way in which the argument just sketched is intended to work (*e.g.* section 20).

But sometimes Lucas uses these arguments in quite different ways. In section 8 'The direction of time', Lucas seems to me unclear as to what he would have us conclude. In the opening paragraph of the section (p. 43) it looks as if he is setting out to show us that the directedness of time is essentially built into our *concept* of it because of the role of directed time in communication. 'Directionless time is not time at all'. Such a conclusion is quite consistent with a view of General Relativity which sees the *t*-dimension as utterly foreign to time as we conceive of it. A similar note is sounded often in Lucas's early pages. However, the conclusion actually drawn seems to be that we live, in fact, in a directed time, and it appears to be drawn from the premiss that communication can and does go on. Of course it no doubt is the case that directed time is a necessary condition of communication perhaps in just the way Lucas shows. But the premiss that people do communicate appears in our theory as a simple, empirical, contingent fact which might have been otherwise and might well be impossible in some region of space-time. That there are people and that they do communicate occupies no privileged status either in the world or in our scheme of concepts. It is quite incapable of yielding us philosophical conclusions of the sort that we cannot *conceive* of directionless time, any more than it can yield the conclusion that we cannot conceive of the world as unpeopled.

Later again, Lucas argues merely for the *pre-eminence* of Euclidean geometry in our conceptual scheme by what (on p. 44) he calls a communication argument. I do not quite see how it is an argument of that kind, in fact, though it is recognisably transcendental. Euclidean geometry is the 'natural' geometry for beings which see, move and grapple with things in their environments. But this argument is said not to show that spaces have to be Euclidean, though it may serve to bring to light hidden assumptions and to suggest new lines of research. So this is a different use of transcendental arguments again. It makes interesting, thought provoking reading.

By contrast, however, (in section 48) we find an explanation offered as to why space should have three dimensions (rather than two, say), this being allegedly difficult to accept as a brute fact (p. 242). Various reasons are given. First, there is an '*ecological* necessity', (p. 243) for planetary orbits would be unstable in spaces of higher dimensionality so that human life and human thought would be impossible. Then there is an argument that, in space of two dimensions, the topology of the cerebral network would be very limited. Thirdly we are offered a communication argument which shows that, apart from communication by travelling objects (letters *etc.*) causally transmitted messages can retain high fidelity only in a space of three dimensions. However dissatisfied one may feel with accepting three dimensional space as a primitive fact about the world it seems far more plausibly fundamental than the facts by means of which Lucas seeks to explain its basis. If anything in the universe is wholly fortuitous and cries out for an explanation of its apparently superfluous presence, it is ourselves. The argument of this section might gain in plausibility were Lucas to give it an explicitly theocentric setting. But he does not. It seems to have no properly transcendental structure at all.

Finally, Lucas offers us a transcendental deduction of the Lorentz transformation by means of communication arguments. The deduction is too intricate to bear exposition in an already long review and I am not quite certain how it is intended to work. But I believe that it has led Lucas to misinterpret the dilatation of time. In section 46, on this topic, he sees the construction of public time from private time (p. 228) or the spatial separation of the origins of inertial frames in relative motion (p. 231) as playing some role in the dilatation of time. This leads him to compare time dilatation with variations in local time in different geographical regions on the surface of the earth (p. 230). It also leads him to speak of the time discrepancies in the clock paradox as caused by accelerations, as revealing that clocks are affected by forces (pp. 234–5). Something is confused in all this. Examples in which several clocks together register retardation without acceleration and arguments that retardation must be a function, not of acceleration, but of uniform motion go back at least to the debate in *Nature* of the fifties. Lucas's claim that the clock paradox is 'not a problem for the philosopher' is, I think, not correct, or, at least, not for the reasons he gives.

Reviewers find their dissent more interesting than their agreement since agreement can prompt only repetition. That much of this review concerns my doubts about this book, especially in respect of its methodology, does not mean that I found little in the book which is valuable. On the contrary, the great range of topics discussed, the novel and provocative approach to them, the lively and lucid prose in which it is expressed, together with the learning and insight Lucas brings to bear on the questions make it a stimulating book to read and an interesting one to review.

GRAHAM NERLICH
University of Adelaide

NERLICH G. [1976]: *The Shape of Space*. Cambridge University Press. £8.50. Pp. 269

The Shape of Space is a work of major importance. It argues an unfashionable thesis trenchantly and convincingly. No philosopher concerned with the nature of space or the status of scientific theories or the particular views of Leibniz, Newton, Hume or Kant, can afford to neglect Nerlich's arguments, and some of his insights are likely to be absorbed into the philosophical tradition. His writing, in welcome contrast to some protagonists in the field, is clear, and he takes care to make his main points intelligible to non-mathematicians without abandoning the rigour of his arguments. Stylistic purists may be put off by a tendency to place connecting particles at the end of sentences, however. But such antipodean inversion seems proper in an author with his point of view, and adds an agreeable nutty flavour to his prose.

Nerlich's main contention is that space is real. 'Space is not a nothing but a something. It is a real live thing' (chapter 7, *fin.*, p. 154). His most telling argument is based on an analysis of the logical concept of space, which shows it to be irreducible to a system of relations. Although some spatial statements may be rendered, reasonably adequately, by relational statements about the distances between material objects, there are others, notably topological statements, which do not admit of any reductive analysis. That space is unbounded, that space is continuous and that space is three-dimensional, are simple examples of topological statements which are essentially *global*: they characterise space as a whole, and for that reason cannot be adequately rendered by a set of statements about the distances between particular things in space. A less well known topological property is orientability, and in chapter 2, 'Hands, knees and absolute space', Nerlich shows that Kant was essentially right in his first thoughts that the incongruity of counterparts constituted a cogent argument for absolute space. For it is only in orientable spaces that enantiomorphs are really distinct. In a non-orientable space, such as a Moebius strip, two apparent enantiomorphs, such as a left-handed and a right-handed L-shaped 'knee', could be superimposed on each other by sliding one of them round the whole strip. We believe—though cosmologists or writers of science fiction may lead us to revise our opinions—that our universe is one in which no continuous path could lead a right hand to be transformed into a left hand; that is to say, we believe that our actual space is orientable; and this is a property of space as a whole, which justifies us in regarding it as a particular, not a logical fiction to be explained away in terms of relational statements that alone are real. 'Space is a particular with a definite structure, a topological one, just like any other particular thing.'

Nerlich devotes much space to countering the conventionalist arguments of Reichenbach and Grünbaum. Their procrustean programmes represent everything except statements about sense-data as non-factual, in which there is no right or wrong, and all we can do is to adopt whatever convention we happen to find convenient. Nerlich uncovers the arguments that can be adduced in favour of conventionalism, and shows them to be misconceived. The theory of knowledge does not support Reichenbach's version, nor does the theory of meaning support Grünbaum's, quite apart from the special problems raised by space. With space, although conventionalism at first sight looks more attractive, with metrical geometries being conventionally adopted variants within a perceptually given topological framework, conventionalism cannot in fact render adequately the topological features of our experience. For what is in issue is not, as Reichenbach supposes, merely local topology, but, as Nerlich establishes beyond doubt, global topology. Besides the question of orientability, there is the question of whether a space is contractible or not, and this cannot be regarded as a mere matter of convention, without introducing causal anomalies and doing violence to our scheme for identifying and reidentifying particulars. The topology of space is intimately connected with other features of our conceptual framework, and has as good a right as they have to be regarded as factual.

Besides the argument from topology, Nerlich argues generally against reductive analyses of space, and in particular against relational theories of spatial measurement. His first chapter, in which he discusses Leibniz's theories in the *Monadology* and the correspondence with Clarke and Kant's theory in the *Critique of Pure Reason*, is illuminating, but will not convince a determined reductionist. It is true, of course, that any reductive analysis of space will, if successful, be somewhat implausible. If I make out space to be purely the creation of the mind or to be simply a system of relations, it will lose the givenness and spaciousness we normally associate with it; but this is a price the reductionist is prepared to pay for the sake of economy or the over-all elegance he hopes to achieve. So, too, although we can agree with Nerlich that 'spatial things are not members of space; they fill space' (chapter 1, section 7, p. 17), his rebuttal of set-theoretical reductionism is not completely convincing. In chapter 8 he argues against Grünbaum's metrical conventionalism and makes some effective points against Grünbaum's less happy speculations. But he relies too much on our having some primitive concept of extension not susceptible of set-theoretical analysis. I think I have some concept of spatial and temporal extension, so that even if I were forced to accept some quantised view of space and time, I should still ask *how long* the hodons and chronons were, and think of them as spatial and temporal *intervals*, not points or moments: that is to say, whatever the physical facts, my *concepts* of space and time are of spatial and temporal *extensions*, not numbers of units. But Hume thought differently. Nerlich rests his case too much on an appeal to my prejudices—and I am disposed to agree with him anyway—and does not recognise how un-primitive and relatively recent our concept of extension is. For this reason also he dismisses too readily the possibility of further elucidation of the concept in terms of measure theory. Certainly such an elucidation need not lead us to conventionalism—we can ascribe to some spaces an intrinsic absolute metric, as he himself argues subsequently, and this will be so, however we analyse that metric in measure-theoretic terms.

In the final two chapters Nerlich deals with General Relativity, in order to scotch the idea, still widespread, that Relativity supports a relational, rather than an absolute, theory of space. Once we drop the distinction between physics and geometry, and replace forces by geodesics in curved space-time, space stops being the stage on which physical things happen, and becomes part of the physical explanation, and to that extent a physical thing itself. As in topology, so again in relativistic physics, explanations are not simply in terms of local relations but involve global features as well. And therefore, once again, the relationist is refuted, and the very arguments he brought forward to demonstrate the relational nature of space establish, instead, its absolute character.

Nerlich has imposed strict limits on what he attempts to do. Within those limits he succeeds admirably. Although others have and will argue in favour of absolute space, few are likely to do so in so lively a way, or to persuade us that space is not only absolute 'but a real live thing'.

J. R. LUCAS
Merton College, Oxford

ROSENKRANTZ, R. D. [1977]: *Inference, Method and Decision*. Dordrecht: D. Reidel $26.00 (paper $11.95). Pp. 262.

When does evidence of observation count in favour of scientific theories or justify their acceptance, and can the criteria of "counting in favour" be given a precise mathematical form? Are these criteria axioms or theorems of the calculus of probability, and if not, how if at all, are they related to it? These are old problems, but over the last three decades ever increasing philosophical work has been devoted to them. Rosenkrantz's book is one more product of this work. The book is subtitled "Towards a Bayesian Philosophy of Science", and so Rosenkrantz holds that work in this field ought to be concerned with "counting in favour" and not with acceptance, and that the probability calculus has a good deal to do with the criteria for the former. He has a powerful and to my mind well aimed attack on acceptance theories in the tradition of Levi's *Gambling With Truth*. He rightly claims that such theories are highly arbitrary. There is no good reason for accepting Levi's measure of epistemic utility, let alone any particular value for the "index of caution" which occurs in this measure. One can give useful content to the notion of "accepting" a theory if accepting a theory is acting on it in a particular situation, or submitting it to further tests, but not if it is a purely theoretical epistemic activity.

There is quite a bit of elegant mathematics in this book, and Rosenkrantz writes from a sophisticated background in statistics. Most of his examples of theories requiring assessment are of the kind dealt with by statisticians—hypotheses about the location of genes or chromosomes, hypotheses about proportions of bad products on factory lines, hypotheses about the proportions of balls in bags and frequencies of heads in coin-tosses. And here Rosenkrantz has many valuable things to say about theories of statistical testing and support. He emphasises how evidence is equally favourable or unfavourable to hypotheses, irrespective of whether or not the investigator has decided on what to do with the evidence (*e.g.* follow a certain rejection-rule for hypotheses) before he has got it. He deals well with such criticisms as that a Bayesian must deny the point that if an investigator has followed a rule of optional stopping (*e.g.* stop when the evidence is of such and such a kind) the results of the investigation are worthless. He shows, on the contrary, that a Bayesian must hold that if such a rule is followed, the number of alternative results of the experiment will be greatly diminished, and hence the evidential value of any particular result will be significantly altered; and that the evidential value of the result will depend on the particular rule of optional stopping followed, and will not necessarily be worthless—all of which shows the Bayesian to be a very reasonable man. Rosenkrantz considers that his general approach is equally applicable to theories about what are the laws of nature in physics or astronomy, but here he is less plausible and the deficiencies of his approach become much more evident. The main example which he takes is the Copernican Theory of astronomy and its supposed advantages over the Ptolemaic. He argues that the advantages of the Copernican theory lay in its simplicity (*e.g.* various phenomena following naturally from the Copernican Theory, which had to be explained in an *ad hoc* way by the Ptolemaic which postulated odd coincidences), and the criteria of simplicity involved here are those which he has purported to show at work in statistics.

For a Bayesian the most important concept is of course that of probability. His interest ought to be in the probability of theories, which is what Bayes's Theorem tells us how to calculate and which determines the expected utility of acts. But, about a third of the way through the book, Rosenkrantz's interest shifts to the concept of "support", and hereafter his concern is with the support which theories obtain from data. A theory's support is basically a matter of its "likelihood" (in the statistician's sense)—how probable it is that the data will occur, if the hypothesis is true. This would be fair enough if the point of calculating support was to calculate probability (for, after all, Bayes's Theorem relates probability to support) but Rosenkrantz seems to develop an independent interest in support, and to neglect probability to such an extent that he almost reaches the Popperian view that scientists ought to prefer improbable theories. This comes about in the following way. First, he considers theories with adjustable parameters (*e.g.* the theory that two variables are related by a second-degree polynomial equation, or the theory that a statistical probability p lies between 0·49 and 0·51). He suggests that we should measure the support for such a theory by its average likelihood, that is the average of the likelihood of its special cases. He then seems to assume that the scientist's task is to choose between theories with adjustable parameters, *e.g.* decide that the equation is a second-degree polynomial, and then opt for one special case of the preferred theory. But why should the scientist set about things in this roundabout way—why should he not go for a specific theory to start with? However, if we suppose that the scientist operates in this way, Rosenkrantz can explain why the scientist does not always end up preferring the complicated theory which predicts the data with perfect accuracy. This is for the reason that it belongs to the wrong family of theories.

Thus in fitting curves to experimental data we might start with the simplest hypothesis that the curve is linear. We can then consider all the different linear hypotheses and compute their average likelihood. We can then compare this with the average likelihood of second-degree, third-degree, *etc.* polynomials. As we move up the polynomials, maximum likelihood will never decrease and will normally increase (*viz.* the best polynomial of degree $n+1$ will fit the data at least as well as the best polynomial of degree n), but so too will what Rosenkrantz calls "sample coverage", that is the probability on a null hypothesis that data will fit the theory. The theory with high sample coverage is one likely to give reasonable predictions, whatever the world is like. When sample coverage increases too much, average likelihood begins to decrease. So if we seek the type of theory with the greatest average likelihood, we can explain not only why we do not always reach the specific theory with the maximum likelihood (*i.e.* the complicated curve which fits the data perfectly) but also why we do not end up with theories with too much sample coverage, an undesirable feature. A theory with lower sample coverage will be a simpler theory, on Rosenkrantz's understanding of simplicity. In this understanding of simplicity, Rosenkrantz is basically in the tradition of Popper, for whom simplicity is a matter of content. The more precise a theory and the larger its scope, the simpler it is. It then follows, as Popper showed, that the theory that a curve is a second-degree polynomial is simpler than the theory that it is a third-degree polynomial. It follows too, to use Rosenkrantz's statistical examples, where p is a statistical probability, "$0{\cdot}04 \leqslant p \leqslant 0{\cdot}06$ is simpler than "$0{\cdot}03 \leqslant p \leqslant 0{\cdot}07$" but less simple than "$p = 0{\cdot}4372$". And so we have a justification of simplicity—a simpler theory which fits the data will have more support than a more complicated theory, because one will be more likely to observe just those data if the simpler (alias more precise) theory is true than if the more complicated (alias less specific) theory is true.

However, the point which Popper made remains—in this sort of sense of simplicity theories are going to be less probable, the simpler they are—other things being equal (although Popper did not add that qualification). This is because their posterior probability depends not only on their support, but on their prior probability, and the prior probability of a hypothesis is greater, other things being equal, the less its content. Hence it is a theorem of the probability calculus that if h entails h', for all e $P(h/e) \leqslant P(h'/e)$. The scientist who seeks for this sort of simplicity is seeking for what is, other things being equal, less probable. Rosenkrantz has two unconvincing pages, attempting to avoid this conclusion which ends with the claim (p. 130) that "theories of minimal sample coverage are preferable just because they are capable of attaining the highest posterior probabilities". That is simply false. Theories of less content which are entailed by theories of minimal sample coverage will always have even greater posterior probabilities than the latter, whatever the evidence.

What has gone wrong? First, with regard to the relation of content and probability—different scientists with different interests will be interested in theories of different amounts of content; one may only want to know a statistical probability within a range, another may wish to know it very precisely. Within the class of theories of a given amount of content, obviously a scientist wants the most probable theory. Secondly, with regard to the determinants of probability, obviously support is one of them, but prior probability is another. Prior probability is not, as Rosenkrantz supposes, solely a function of lack of content. (Rosenkrantz gives a useful precise measure of the latter in terms of entropy.) It also depends on simplicity of a very different kind from that which Rosenkrantz picks out. Intuitively, to use Jeffrey's example, $5x = t^2$ is a far simpler formula relating x and t than is $5x = t^2 + t(t-5)(t-10)(t-15)(t-20)(t-25)(t-30)$. If these are two formulae received equal support from data (*i.e.* were equally successful in predicting the data), nevertheless the former would be regarded as more probable. It has greater prior probability because it has greater simplicity. It has greater simplicity, but not because it has greater content, for it does not—both formulae make equally precise predictions. This sort of simplicity is rather a matter of paucity of variables, and simplicity of kind of mathematical relationship. But there is no reason to regard this simplicity as mediated *via* the simplicity of the type of equation to which each belongs (*e.g.* the second equation as being a seventh-degree polynomial), and the latter as dependent on the "sample coverage" of that kind of equation. For low sample coverage may make for high support, but it does not make for high probability. Simplicity is not just a matter of lack of content, and Rosenkratz has got in a bad muddle by supposing that it is.

If Rosenkrantz had taken more examples from physics and less typical statistician's examples, he might have seen this. For it is typical of the kind of hypothesis with which statisticians deal that they do not differ greatly from each other in simplicity of the kind which I have just delineated. "$p = 0{\cdot}43$" is hardly more or less simple in this respect than "$p = 0{\cdot}4435$" or "$0{\cdot}42 \leqslant p \leqslant 0{\cdot}44$"; although the last has much greater prior probability than the first two because of its less precise character. Physicists however often construct hypotheses (and could easily construct a very large number of hypotheses) equally successful in predicting observations so far, but differing in the number and kind of variables postulated, and the mathematical relations holding between them. Most of these they would never seriously think of putting forward, but the reason is that these are not simple enough in my sense.

R. G. SWINBURNE
University of Keele

MANNINEN, J. and TUOMELA, R. (*eds.*) [1976]: *Essays on Explanation and Understanding*. Dordrecht: D. Reidel. Pp. 440.

In his [1971] book G. H. von Wright was primarily concerned to defend two theses. The first was that the explanation of an agent's actions in terms of his intentions conforms to a distinctive non-causal pattern of explanation (teleological explanation). The second was that the notion of cause itself depends conceptually on the idea of human action. A subsidiary theme of the book was that the human sciences, and in particular history, are 'quasi-causal' disciplines, in which teleological understanding plays an essential part in linking sequences of natural causation.

Von Wright argued that the basic procedure for explaining an action was (roughly) the reconstruction of the following kind of practical inference:

$$\frac{\begin{array}{l}A \text{ intends to bring about } p\\ A \text{ considers that he cannot bring about } p \text{ unless he does } a\end{array}}{\text{Therefore, } A \text{ sets himself to do } a.}$$

He denied that this was a schema of causal explanation on the grounds that the explanans is not linked to the explanandum by any empirical generalisation. On the contrary, he took the link to be conceptual. The premisses of the inference *entail* the conclusion, and, correlatively, cannot be established independently of our establishing the conclusion.

Causal explanation itself depends on the distinction between nomic generalisations, which support counterfactuals, and accidental generalisations, which do not. Von Wright argued that this distinction can only be understood in terms of a prior understanding of the possibility of human intervention in nature: one event is nomically antecedent to another just in case our having acted to bring about the former, on an occasion when it was in fact absent, would have eventuated in the occurrence of the latter.

Essays on Explanation and Understanding consists of fourteen papers addressed to the issues raised by von Wright's book, together with replies to these papers by von Wright himself, and a new essay 'Determinism and the Study of Man' in which von Wright somewhat modifies his position.

The first three papers are concerned with the broader issues of the status and subject matter of the human sciences. Manfred Riedel applauds the removal of history from the natural realm, and relates this to Hegel's teleological conception of historical development. But he suggests that Hegel's teleology is overly 'technical', explaining only the means and not the end, and recommends a return to Kant and the transcendental critique of historical methodologies. Riedel seems here to be sympathetic to 'critical theory', the contemporary German synthesis of Hegelian neo-Marxism with theories of the interpretative understanding of human action. This affiliation is more explicit in the paper by Rüdiger Bubner. Following recent writers like Habermas, Apel and Gadamer, Bubner suggests that a 'transcendental hermeneutics' can function simultaneously as a means for the interpretative understanding of human history and as an epistemological metatheory which comes to terms with the fact that all knowledge is historically conditioned. Maria Makai criticises von Wright from a more orthodox Marxist perspective. She objects to his dualism about man and nature. In her view a complete understanding of human action in its social and material context would transcend the traditional mind-matter dichotomy.

These first three papers are somewhat dense and likely to prove obscure to readers reared in an analytic environment. It would be unfortunate if they were thereby discouraged from continuing, as there is much that is worthwhile and accessible in the remaining contributions.

Most of the remaining papers focus on von Wright's scheme of intentionalist explanation. Jaegwon Kim discusses an apparent counter-example to this scheme originally raised by Nick Sturgeon: Jones is going to read a philosophy paper at the University of Cincinatti, and, since he is going to Cincinatti, he plans to look up his in-laws there. Now,

> Jones intends to read a paper at the University of Cincinatti, and
> Jones believes he cannot do this unless he goes to Cincinatti

Together these seem to explain quite adequately why

> Jones sets himself to go to Cincinatti

But it is equally true that

> Jones intends to visit his in-laws, and
> Jones believes he cannot do this unless he goes to Cincinatti

Yet these facts do *not* seem to explain why

> Jones sets himself to go to Cincinatti.

Von Wright in his reply to Kim tries to deal with the Sturgeon counterexample by means of a distinction between conditional and unconditional intentions. Someone who unconditionally intends something is committed to doing whatever is required if he is to do that thing. Having a conditional intention to do something if and when certain conditions obtain does not, on the other hand, commit the intender to doing anything before those conditions are satisfied; in particular it in no way commits him to bringing about those conditions. Only unconditional intentions are qualified to explain the actions believed necessary to their fulfilment.

So the idea is that Jones only conditionally intends to see-his-in-laws-if-and-when-he-is-in-Cincinatti. Consequently this intention cannot feature in the explanans of a teleological explanation. The intention to read his paper, on the other hand, is unconditional, and so can explain his doing what is required to ensure this.

This response to the Sturgeon counterexample is inadequate. For Jones's intention to see his in-laws does explain quite happily his doing some of the things he believes necessary thereto, such as getting their phone number from his wife before he leaves. If von Wright is correct to distinguish between conditional and unconditional intentions, and to deny any explanatory power to the former, then Jones must presently intend unconditionally to see his in-laws in Cincinatti.

Of course there is still something dodgy about Jones's intention to see his in-laws, by comparison with his intention to read his paper. Jones's going to Cincinatti is essential to the fulfilment of either of these intentions, yet only the intention to read his paper seems to give an explanation of why he does so. But this is simply the asymmetry that Sturgeon raises, and which von Wright needs to give some account of.

Kim attempts to save von Wright's schema by attending to the way in which intentions are generated. He thinks that the way an intention stems from an agent's beliefs and wants is important for its explanatory significance. To deal with the Sturgeon case Kim proposes that if a given intention results from a belief that p, then that intention cannot explain the agent's intending to or setting himself to bring about that p. Jones's intention to see his in-laws, unlike his intention to read his paper, *results* from his belief that he is going to Cincinatti, and so cannot explain his going there.

In reaching this conclusion Kim is hinting at an alternative line of approach to action explanation. If the explanatory power of an intention depends on the way it issues from the agent's beliefs and wants, why bring in intentions at all? Why not simply explain actions as issuing directly from the agent's beliefs and wants? The distance between such an approach and the one von Wright actually has in mind is shown in his replies to those several contributors who do take him to be discussing the way actions issue from beliefs and desires, and criticise him accordingly. Somewhat surprisingly, von Wright does not resist their arguments. He merely observes that they are concerned with processes of deliberation and decision, whereas his problem was solely the further link between the formation of an intention and the ensuing action.

It is doubtful whether von Wright can legitimately restrict his brief in this way. In the end what the Sturgeon counterexample seems to show is that the route to the explanation of actions does not proceed *via* intentions at all, but comes directly from beliefs and wants. Raimo Tuomela suggests at the end of his contribution that the Sturgeon case will cease to be a problem once we embrace the idea that actions depend causally on beliefs and desires. He does not spell this out in any detail. But (without worrying too much about the rather unfashionable causal aspect of his suggestion) it is easy enough to see how it would work.

Our understanding of the way actions depend on beliefs and desires tells us that Jones's going to Cincinatti depends on his desire to read his paper there, but not on his desire to see his in-laws. For, if he lacked any desire to read his paper there, he would not go. But, by contrast, he would still be going even if he had no desire to see his in-laws. This asymmetry does not stand if we replace 'desire' by 'intention'. It is easy enough to account for the asymmetry Sturgeon raises once we take beliefs and wants, rather than intentions, to be the primary explainers of actions.

From this point of view intentions as such do not explain actions. To say a man intends something is to say he desires that thing, either as an end or as a means, and that he is going to perform actions which he believes will further the satisfaction of that desire. The significance of the Sturgeon case is that it shows that those actions will not always depend counterfactually on that desire (even if this is so in most cases, which of course creates the impression that there is a direct explanatory link between intention and action). No doubt it would be possible to amend von Wright's scheme, *à la* Kim's modification, to restrict it to those intentions which 'point to' attitudes on which the relevant actions do depend. But this would be a quite *ad hoc* way of avoiding the conclusion that the explanation of action need not bring in intentions at all.

Let us now turn to von Wright's 'interventionist' analysis of causation. The most incisive comments on this are made by Peter Winch. Winch reconstructs von Wright's standard procedure for discovering causes as follows. We have a system in state α, which we assume will not change to a, nor subsequently display p, unless we change it to a. We change it to a, and p follows. This entitles us to the conclusion that a causes p. For we have established the relevant counterfactual that p wouldn't have been if a hadn't been. But this counterfactual presupposes the further counterfactual, that if we hadn't acted, a would have been absent. And this latter counterfactual is not a causal one, as our acting does not *cause* what we do. So causal judgments depend upon an *a priori* understanding of human actions.

Winch observes a number of faults in this. First, the crucial original assumption, that α won't change to a or p unless we intervene, itself seems to be a causal judgment. And this judgment seems independent of any of our ideas about human intervention. The fact we can intervene to produce a is irrelevant, since what we are after is what happens when we don't intervene, and if we are going to find this out from anywhere we will have to find it by just observing. Secondly, even if we allow, with von Wright, that our acting doesn't cause what we do, it by no means obviously follows that the connection between our *not* acting and what then happens is not a causal connection. So it is not obvious that 'If we hadn't acted, a would have been absent' is not a causal counterfactual. And, finally, it is quite unclear in exactly what sense this counterfactual is presupposed by the target causal counterfactual, that p wouldn't have happened if a hadn't.

Von Wright replies to the second of these criticisms by arguing that while causal counterfactuals state what alteration would have been produced if some change had occurred, 'action' counterfactuals state how things would have remained the same had we not done anything. This does not seem an adequate basis for the distinction he needs. Von Wright does not reply to Winch's other criticisms.

Of the other papers in the volume, those by Alasdair MacIntyre, Lars Hertzberg, Frederick Stoutland and Rex Martin discuss further the topics already mentioned. The rest are on topics only distantly related to von Wright's book.

In a paper entitled 'An Analysis of Causality' Aleksandar Kron develops some ideas originally put forward by H. A. Simon. Simon has shown how it is possible to define an asymmetrical cause-effect relation amongst the variables in certain solvable sets of linear equations. His approach explicates our intuitive notions about which variables are dependent on which. (Roughly, in the kind of system in question we can partially order the variables according to which other variables they appear with in equations. A variable which occurs in an equation which contains no other variables is independent of all others. A variable which occurs in an equation in which the only other variable is such an 'absolutely independent' variable, depends on the latter but is independent of all others. And so on.)

What Kron does is apply Simon's methods to the set of free variables in certain systems of well-formed formulae in the language of first-order logic. While this makes for a somewhat forbiddingly technical paper, it is of interest in suggesting that Simon's ideas might be of more general relevance than indicated in his original paper. On the other hand, it is most doubtful whether this should qualify as 'an analysis of causality'. Simon's techniques are intended to explicate the direction of causation amongst a set of variables that are already assumed to be nomically related. This is indeed an important problem in the theory of causation (and, incidentally, as Tuomela points out, one that von Wright's interventionist account completely fails to come to grips with). But, as Simon makes clear in his original paper (Simon [1953]), our ability to specify the direction of causation amongst the variables in some system of equations has to rest on an independent understanding of which variables depend on which. This is to do with the simple point that a given system of solvable equations can be reorganised by elementary row transformations without changing the solutions. These alternative ways of writing the equations will, however, present different relationships of relative dependence and independence amongst the variables in question. Simon argues that our choice amongst the various equivalent ways of writing the equations reflects our prior judgments about which variables would be affected and which unaffected were one or more of the non-zero coefficients in a given equation in the system to be altered. Clearly this means that Simon's work fails to yield a philosophical explanation of the direction of causation; nor does Kron present any reason to think his generalisation of Simon's work is an improvement in this respect.

The real interest of Simon's work lies in the connection it uncovers between causal ordering and 'identifiability'. A structure of equations is identifiable if we can identify certain fixed coefficients from a number of observations of the values of the variables on particular occasions. What Simon shows is that the identifiability of a structure of equations requires that its variables be causally ordered; that is, that we know beforehand that the equations will represent a certain order of relative dependence and independence amongst the variables. This is of particular interest in connection with stochastic equations and the estimation of their coefficients from statistical data: work such as Simon's has been valuable in making clear exactly how much has to be assumed beforehand if we are to make meaningful use of multiple regression techniques. Kron says nothing about whether similar considerations arise with his generalisation of Simon's work. It is not obvious to me that they do.

Ilkka Niiniluoto is concerned to defend 'inductive-statistical' explanations against those critics who hold that such 'explanations' can only show that an event was to be expected, not *why* it actually occurred. To this end Niiniluoto follows Wesley Salmon in arguing that genuine inductive explanations are appropriate precisely to those events which are not completely determined. Such explanations are then inductive, not because we are ignorant of the factors that determine the explanandum, but simply because the full set of factors relevant to its occurrence fails to determine it. Niiniluoto then argues that this inductive pattern of explanation is appropriate to the explanation of historical events and of human actions in general. The difficulty with this is that in the kind of social scientific case Niiniluoto has in mind it is generally absolutely obvious that our explanations are incomplete—that the inclusion of (for practical reasons) unknown factors would alter the probability of the explanandum. The moral of this is not, I think, that such inductive explanations do not explain—often they patently do. Rather it is that Niiniluoto and Salmon are wrong to think that inductive explanation requires indeterminism. It would be much closer to the mark to see an inductive explanation as satisfactory precisely insofar as it succeeds in picking out some part of a set of factors which fully determines its explanandum.

Anthony Kenny contributes a precise and sophisticated analysis of the logic of the 'can' of human ability. He shows that this cannot be represented as a possibility operator in any of the standard modal systems, since it fails to distribute over disjunction. A standard person can either pick a red card or a black card from a standard pack; but it does not follow that either he can pick a red card or he can pick a black card. In the end Kenny suggests that the impossibility of representing this 'can' as a standard modality lends support to a conditional analysis of human abilities. Though Kenny does not go further, it is significant that such a conditional analysis would flow naturally from the view that actions depend on beliefs and desires: 'A can Φ' could in the first instance be read simply as 'A will Φ, if his beliefs and desires indicate Φ-ing as the optimal thing to do'.

Jaako Hintikka examines the notion of intentionality. He rejects the traditional phenomenological view that what makes an act intentional is its being directed towards some content, and instead defends the equation of intentionality with intensionality, which he explicates as involving the simultaneous consideration of several possible worlds. Hintikka has interesting comments on Husserl's ideas and on possible world semantics. How seriously one takes the overall thesis depends on how plausible one finds his possible worlds. I wondered whether these were supposed somehow to underlie socially objective languages, or whether they were constituted by the subjective conjectures of individuals.

Either line raises problems: the former kind of possible worlds would seem to be unwarranted abstractions, while the latter kind would derive from certain of an individual's intentional acts, and so, as J. N. Findlay points out in a comment on Hintikka's paper, be ineligible for explaining away intentionality.

In 'Determinism and the Study of Man' von Wright refines his views on the intentional explanation of action; and he expands on the connection between such explanations and social phenomena like communication, norms and institutions. The latter observations are interesting enough. But there is nothing in the former remarks to remove the suspicion that it is a mistake to try to explain actions in terms of intentions.

DAVID PAPINEAU
University of Reading
Macquarie University

REFERENCES

WRIGHT, G. H. VON [1971]: *Explanation and Understanding*. London: Routledge and Kegan Paul.

SIMON, H. A. [1953]: 'Causal ordering and identifiability' in W. C. Hood and T. C. Koopmans (*eds.*): *Studies in Econometric Method*. New York: John Wiley.

The Journal of Philosophy

Subscriptions at $10.00/year; $7.00 to students
$16.00 to libraries and institutions

The *Journal of Philosophy* announces the publication of *Dewey and His Critics,* edited by Sidney Morgenbesser. From 1905 until his death, John Dewey published some of his most significant papers and replied to some of his most important critics in the pages of *The Journal of Philosophy.* These exchanges tell the story of the development of his philosophy over 50 years and the formation of American philosophy through that period. *The Journal of Philosophy* presents in one volume Dewey's seminal papers, the critical comments, and his answers to such critics as Russell, Lewis, Santayana. The Introduction by Sidney Morgenbesser situates Dewey and his critics in the perspective of contemporary philosophical thought. 760 pages. $8.00 in paper, $15.00 in cloth, plus 60¢ postage.

720 PHILOSOPHY HALL, COLUMBIA UNIVERSITY, NYC 10027

Philosophical Studies

An International Journal for Philosophy in the Analytic Tradition

Editor: Keith Lehrer, University of Arizona, U.S.A.

Editorial Assistant: Vincent Lazara, University of Arizona, U.S A.

Board of Consulting Editors: David Braybrooke, Joseph L. Camp, Jr., L. J. Cohen, James Cornman, Alan Donagan, Keith Donnellan, Mary Hesse, Risto Hilpinen, R. M. Martin, Terence Parsons, Gunther Patzig, A. M. Quinton, Jerome Schneewind, Robert Sleigh, Jr., Judith Thomson.

Philosophical Studies fills the urgent and growing need for space devoted to quick publication of timely contributions to analytic philosophy. It emphasizes the rapid exchange of notes and criticism To provide balance and scope the editors will, on occasion, solicit contributions. comments and elucidations from philosophers whose voices might not otherwise be heard when the spotlight of discussion points in their direction.

Philosophical Studies welcomes papers in all areas of analytic philosophy. It recognizes that there are divergent 'tendencies' within the analytic movement, and requires only that contributions represent an application of methods of analysis and clarification of meaning. Papers the content of which falls primarily within the scope of the natural, social or psychological disciplines will be considered only if this content is subordinated to the purposes of philosophical analysis.

Subscription price per volume of six issues Dfl. 95,– / US $38.00, plus Dfl. 20,- / US $8.00 for postage and handling. Two volumes will be published yearly. Personal subscription price on request.

D. Reidel Publishing Company

Dordrecht - Holland / Boston - U.S.A.

The Monist *An International Quarterly Journal of General Philosophical Inquiry*

Founded 1888 by **Edward C. Hegeler** *Editor*, Eugene FREEMAN

Editorial Board: William P. Alston, Monroe C. Beardsley, Lewis White Beck, William A. Earle, Dagfinn Føllesdal, William Frankena, Maurice Mandelbaum, R. Barcan Marcus, Richard Martin, Mary Mothersill, Joseph Owens, Richard Rorty, J. B. Schneewind, Wilfrid Sellars, John E. Smith, Richard Wasserstrom.

Managing Editor, Sherwood J.B. SUGDEN

EACH ISSUE IS LIMITED TO ARTICLES ON A SINGLE GENERAL TOPIC. Communicate with the Editor in advance for Special Instructions. Papers should be 4000-8000 words in length and must be submitted in duplicate nine months prior to the scheduled publication of the issue, accompanied by return postage.

SCHEDULED PUBLICATION DATES:

Vol. 62,	No. 2	Apr.,	1979	Truth, Meaning, and Reference
Vol. 62,	Vo. 3	July,	1979	The Concept of a Person in Ethical Theory
Vol. 62,	No. 4	Oct.,	1979	Objectivity in Knowledge and Valuation
Vol. 63,	No. 1	Jan.,	1980	The Carus Lectures of William Frankena (By Invitation Only)
Vol. 63,	No. 2	Apr.,	1980	Crime, Punishment, and the Quality of Life
Vol. 63,	No. 3	July,	1980	The Relevance of Charles Sanders Peirce
Vol. 63,	No. 4	Oct.,	1980	Philosophy as Style, Literature as Philosophy
Vol. 64,	No. 1	Jan.,	1981	The Carus Lectures of Wilfred Sellars (By Invitation Only)

Editorial Office: Box 1908, Los Gatos, California 95030
Business Office: Box 600, La Salle, Illinois 61301

SUBSCRIPTION RATES: United States: Annual (4 issues): Institutions, $18.00, Individuals, $12.00. Single copies: $4.50. Foreign postage: Add 25 cents to single copy rate or $1.00 to subscription

---

NOTRE DAME JOURNAL OF FORMAL LOGIC

Bolesław Sobociński, *Editor*

Michele Daugherty, *Business Manager*

Published quarterly by the University of Notre Dame in January, April, July, and October of each year, the Journal is devoted primarily to logic and the foundations of mathematics (i.e., algebraic logic, classical logical systems, constructive mathematics, methodology of deductive systems, model theory, non-classical formal systems, proof theory, recursion theory, and set theory).

Articles for publication and all editorial correspondence should be sent to the *Editor, P.O. Box 28, Notre Dame, Indiana 46556, U.S.A.*

Effective 1 July 1975 and beginning with Volume 17 (1976), subscriptions are $20.00 per year for institutions and $15.00 for individuals. Back volumes are $10.00 each for Volumes 1-9, $16.00 for Volumes 10-16. Orders for subscriptions and back volumes (accompanied by payment), and changes of address should be sent to the *Business Manager, P. O. Box 5, Notre Dame, Indiana 46556, U.S.A.*

EDITORIAL BOARD

R. B. Braithwaite
G. Buchdahl
L. J. Cohen
H. Dingle
Sir John Eccles
M. B. Hesse
Sir Harold Jeffreys
W. C. Kneale
S. Körner
J. L. Mackie
D. J. O'Connor
Sir Karl Popper
H. R. Post
J. W. N. Watkins
G. J. Whitrow
J. H. Woodger

EDITORIAL ADVISORY PANEL

The Editors are helped by members of the Editorial Board, members of the Committee of the British Society for the Philosophy of Science and by an Advisory Panel which includes:

Peter Clark
Jon Dorling
Arthur Fine
Michael Gardner
Donald Gillies
Ian Hacking
Colin Howson
Clive Kilmister
Noretta Koertge
Alan Musgrave
Vernon Pratt
Ian Roxburgh
Peter Urbach
Elie Zahar

THE BRITISH SOCIETY FOR THE PHILOSOPHY OF SCIENCE

Committee 1978-9

President: Mr L. J. Cohen
President Elect: Dr G. Buchdahl
Hon. Secretary: Mr A. Young
Hon. Treasurer: Mr D. W. Miller
Members: Mr D. Hirschmann, Dr M. L. G. Redhead, Dr Y. Wilks, Dr P. M. Williams

The British Journal for the Philosophy of Science is the official organ of the Society, whose purpose is described by the following statement from its Constitution and Rules :

' The purpose of the Society is to study the logic, the method and the philosophy of science as well as those of the various special sciences, including the social sciences.'

The Society holds meetings, at which papers are read and discussed, once a month during the academic year. These are held, by kind permission, in University College, London. The Committee may recognise Regional Groups of Members formed to hold meetings in other parts of the country. Any Regional Group so recognised may nominate a representative Member to the Committee.

Ordinary membership is open to all interested persons on payment of the annual subscription. Membership of the Society is international and is by no means confined to people resident in the United Kingdom The annual subscription is £2.50 for U.K. residents and £1.25 for overseas residents ($2.50 if resident in U.S.A. or Canada). Those who wish to join should write to the Hon. Secretary, Mr Alistair Young, School of Social Sciences, University of Sussex, Falmer, Brighton BN1 9QH.

Brit. J. Phil. Sci. Vol. 29 No. 3 September 1978

The British Journal for the Philosophy of Science

Contents

Articles

213–222 JAMES MOOR Three Myths of Computer Science
223–242 GERHARD WASSERMANN Testability of the Role of Natural Selection within Theories of Population Genetics and Evolution

Discussions

243–248 WILLIAM BERKSON Reply to L. Pearce Williams
248–252 JOSEPH AGASSI Williams Dodges Agassi's Criticism
252 L. PEARCE WILLIAMS Reply to Agassi and Berkson
252–256 ARTHUR MILLER A Reply to 'Some New Aspects of Relativity: Comments on Zahar's Paper'
256–262 EDWARD REED Group Selection and Methodological Individualism: A Criticism of Watkins
262–263 PAUL HORWICH A Peculiar Consequence of Nicod's Criterion

Review Articles

265–273 JOHN BELOFF Is Mind Autonomous? (review of K. R. Popper and J. C. Eccles: *The Self and Its Brain*)
274–280 COLIN HOWSON The Prehistory of Chance (review of I. Hacking: *The Emergence of Probability*)
280–283 WLADYLAW KRAJEWSKI Reduction in Physics (review of R. M. Yoshida: *Reduction in the Physical Sciences*)

Reviews

285–310 RON YOSHIDA on W. Krajewski: *Correspondence Principle and Growth of Science*; WILLIAM NEWTON-SMITH on P. C. W. Davies: *Space and Time in the Modern Universe*; D. W. SCIAMA on J. Narlikar: *The Structure of the Universe*; GRAHAM NERLICH on J. R. Lucas: *A Treatise on Time and Space*; J. R. LUCAS on G. Nerlich: *The Shape of Space*; R. G. SWINBURNE on R. D. Rosenkrantz: *Inference, Method and Decision*; DAVID PAPINEAU on J. Manninen and R. Tuomela (*eds.*): *Essays on Explanation and Understanding*

MAR 1979

Printed in Great Britain at the University Press, Aberdeen

Volume 29 Number 4 December 1978
BJPIA5 29 (No. 4) 311-413 (1978) ISSN 0007-0882

The British Journal for the Philosophy of Science

Published for the
British Society for the Philosophy of Science
by Aberdeen University Press

THE BRITISH JOURNAL FOR THE PHILOSOPHY OF SCIENCE

Editors: JOHN WATKINS and JOHN WORRALL

Subscriptions from January 1978
The *Journal* is published quarterly. The subscription price for mailing addresses in Britain is £7.00 net (U.S.A. and Canada U.S. $22.00, other overseas £11.00) per volume post free; single issues £2.50 net (U.S.A. and Canada U.S. $8.00, other overseas £4.00) plus postage. For members of the British Society for the Philosophy of Science—who should order direct from the publisher—the subscription price is £4.50 (U.S.A. and Canada U.S. $14.00, other overseas £7.00). Membership of the Society is international and is by no means confined to people resident in the United Kingdom. Orders may be sent to any bookseller or direct to Aberdeen University Press, Farmers Hall, Aberdeen, Scotland AB9 2XT.

Notes to Contributors
Articles, correspondence and books for review should be sent to John Worrall at the Department of Philosophy, London School of Economics, Houghton Street, Aldwych, London WC2A 2AE. Articles should be accompanied by abstracts of about 100 words.

Material is accepted for publication in the *Journal* on the condition that copyright is assigned to the British Society for the Philosophy of Science.

Three copies of all contributions should be sent, typed on one side only with wide margins and double-spaced. Footnotes, which should be as few as possible, should be typed separately, double-spaced at the end of the text. They should be referred to by consecutive numbering throughout the text, in which they will be inserted in page proof. Bibliographical references should be listed alphabetically at the end. The style of references should follow current *Journal* practice.

Long quotations are printed in small type without quotes, and should be so marked in the typescript. Single quotes should be used to form names of words and for shorter quotations; double quotes for other purposes. Diagrams, tables, and illustrations should be on separate sheets, with their desired position in the text clearly indicated. Symbolic formulae appearing as separate lines should be clearly distinguished in the text, and foreign letters (Greek, German, etc.) indicated in the margin. Material to be printed in italics, especially English letters used as symbols ('*A*', '*x*', etc.), should be underlined. On matters of style and layout, authors should consult CHAUNDY, T. W. *et al.* [1964]: *The Printing of Mathematics*. London: Oxford University Press.

Offprints of articles may be ordered when corrected galley proofs are returned. Fifty are supplied free ; the price of extra copies is given on the order form.

© British Society for the Philosophy of Science 1978

For permission to reproduce material from *The British Journal for the Philosophy of Science*, please apply to the Editors.

Back Volumes
Volumes 1-16 may be obtained from Kraus Reprint Ltd., FL-9491 Nendeln, Liechtenstein or Kraus Reprint Corporation, 16 East 46 Street, New York, N.Y. 10017. Volumes 17-27 may be obtained from Aberdeen University Press, Farmers Hall, Aberdeen, Scotland AB9 2XT. Prices on application.

Brit. J. Phil. Sci. 29 (1978), 311–323 *Printed in Great Britain*

Did Einstein need General Relativity to solve the Problem of Absolute Space? Or had the Problem already been solved by Special Relativity?

by JON DORLING

Introduction
1 *The problem concerns absolute accelerations*
2 *An incorrect epistemological objection*
3 *Einstein's correct objection*
4 *Should we change dynamics or kinematics?*
5 *The Special Relativistic solution*
6 *The identical General Relativistic solution*
7 *The irrelevance of fictitious gravitational forces*
8 *The irrelevance of the neo-Machian programme*
Literature

INTRODUCTION

According to a standard view, Einstein's Special Theory of Relativity left the problem of absolute space untouched and it required the General Theory of Relativity for its solution. Einstein himself seems to have held this view. I shall argue that it is a mistaken view. I shall try to give what I take to be a correct account of the logical situation. It will turn out that Einstein, to an appreciable extent, underestimated the philosophical achievement of his Special Theory. I am quite sure that I am not alone in my heterodox position on this issue, but the standard view has gained very wide currency and deserves to be more openly challenged than it has been in the literature.

1 THE PROBLEM CONCERNS ABSOLUTE ACCELERATIONS

As is well known, the problem of absolute space is really the problem of absolute accelerations. Newtonian mechanics seems to require that

Received 1 *August* 1977

if a body is moving with uniform speed in a straight line, then we need invoke no forces in order to explain its motion; but if, on the other hand, its speed is not uniform, or it is not moving in a straight line—in other words if its motion is accelerated in any way—we are required to invoke forces in order to explain its motion. These forces cannot be prescribed arbitrarily but must satisfy certain conditions; for example, they must satisfy the law of equality of action and reaction. (This law does not carry over into Special Relativity as it stands, but an analogous requirement can be formulated for the permitted forces in that theory.)

2 AN INCORRECT EPISTEMOLOGICAL OBJECTION TO ABSOLUTE SPACE

Now one traditional philosophical criticism of this Newtonian prescription is that, since all motion is relative, we must have some independent way of singling out a frame of reference relative to which the motions are uniform or accelerated before we apply the prescription. Thus it is sometimes suggested that these accelerations should all be read as accelerations relative to some such reference system as that of the fixed stars, and that if there were no such standard reference system available—for example if the solar system were encircled by a uniform cloud of dust which obscured our vision of the stars—then the Newtonian prescription would be physically empty. I do not think that this purely epistemological objection to the Newtonian prescription is really correct. For all that Newtonian physics really requires is that there should be some mathematically definable system of reference in which the Newtonian prescription is satisfied, not that such a frame should be physically instantiated by any actual set of bodies. The requirement would still be empty if there were only one body in the universe, but it is not empty if there are many bodies. In the latter case it is a completely non-trivial empirical discovery that one can in fact define a frame of reference in which the Newtonian prescription is satisfied. It is not satisfied in a frame of reference in which the earth is stationary, for the fictitious forces which we must then introduce in order to conform with $F = ma$ do not then satisfy the principle of equality of action and reaction. It is not satisfied in a frame of reference in which the sun is stationary, for then the forces that the sun exerts on the planets would not be balanced by equal and opposite forces on the sun, without violating $F = ma$ for the sun. But the observed *relative* velocities and accelerations of all these bodies (and it is only these *relative* motions that we can observe) are such that, without reference to any other body, we can mathematically single out a frame of reference (in fact a unique class of frames of reference) in which the Newtonian prescription can be satisfied. The determination of the actual accelerations of bodies relative to such a frame of reference is a matter of mathematical computation rather than of measurement. The adjective 'absolute' in this context is unfortunate. It would be better to call these accelerations the 'force-frame' accelerations. The statement that there exists such a frame, *i.e.* that it is mathematically possible to define such a frame, is really a statement about the observed relative velocities and relative accelerations of the various bodies; it is a statement which imposes substantial empirical constraints on these observed relative motions. Such a theory does not require us to be able to measure anything that we cannot measure, or to give a meaning to any expression which can have no meaning: there is therefore no genuine purely logical and epistemological objection to it. Einstein's own (correct) objection to absolute space was a more subtle one.

3 EINSTEIN'S CORRECT OBJECTION

The true physical objection to Newtonian absolute space, that is to say to the role of absolute accelerations in Newtonian mechanics, seems to be not that the theory is empirically vacuous but that it has an intrinsic explanatory weakness. It always explains slightly less than it purports to explain. However many effects it explains, there will always be some comparable effect that it fails to explain in a non-circular way, since some dynamical effect must be utilised in order to identify the privileged dynamical reference frames. The theory thus violates the principle that to *every* observable difference in effects there should correspond a discoverable difference in the causal conditions of those effects. This objection was most clearly identified and stated by Einstein himself: he presented it in detail both in his popular books and in his 1916 paper on 'The Foundations of the General Theory of Relativity'. The principle that is violated is thus an epistemologically tough-minded version of the ***principle of sufficient reason*** (a principle closely related to the principle of physical determinism). It will be violated somewhere in any application of Newtonian mechanics, for example in the application to the solar system. But the conflict with the principle is best illustrated in idealised situations involving only two bodies.

Consider, in an otherwise empty universe, two approximately spherical bodies, either of which, judged by an observer on the other, is rotating with constant angular velocity about the axis joining the bodies. Now suppose that one body, on closer examination, proves to be an ellipsoid of revolution. Now the Newtonian can give an account of this difference by supposing that the former body is not rotating relative to absolute space, *i.e.* relative to the class of mathematically definable dynamically privileged frames, whereas the latter body is rotating relative to such frames. In the latter case the particles of the body are ascribed absolute accelerations which vary with their positions in the body, and the figure of equilibrium, in which these accelerations are balanced by the excess of the inter-particle forces, is no longer a sphere but an ellipsoid of revolution. Suppose that this Newtonian account is, as far as it goes, consistent with the observations. But then we can still raise the objection—and Einstein does—that this explanation of the phenomena is defective, for the alleged difference in causal conditions—the presence or absence of rotation relative to the dynamically privileged frame—which purports to explain the difference in effects, is not independently discoverable. So Einstein's version of the principle of sufficient reason is violated.

4 SHOULD WE CHANGE DYNAMICS OR KINEMATICS?

Now this problem, which Einstein so succinctly identified, would disappear provided we had a way of distinguishing between uniform and accelerated motion which was independent of the dynamical effects of the latter. Provided we had some independent non-dynamical criterion for discovering whether the particles on the equator of the second sphere were accelerating and by how much, then the principle of sufficient reason would no longer be violated. Now the distinction between uniform and accelerated motion seems properly to belong to kinematics, so first it seems that Newtonian kinematics ought to help us. One might suppose that mere measurements with rods and clocks ought to tell us whether a particle is accelerating or not. For example one could use such measurements to plot the trajectory of the particle in four-dimensional Newtonian space-time: if the result was a curved line the particle's motion would be accelerated; if it were a straight line the particle's motion would be uniform and unaccelerated. All one needs then is a criterion for distinguishing between straight and curved lines which is independent of the particular initial choice of coordinate system in Newtonian space-time, and which does not presuppose that the coordinate axes themselves, in particular the time axis, be straight lines according to that criterion. Now in ordinary Euclidean geometry a straight line can be defined as a line of shortest distance between any of its points. This definition is coordinate-free and perfectly compatible with a curvilinear coordinate system. Could one not provide something analogous in Newtonian space-time? Spatial distance will do as a distance measure between simultaneous events, but it won't do between non-simultaneous events, because the spatial distance between two events can be made as large or as small as we like by measuring it in an appropriately moving frame of reference. The time interval between the two events is however a function which is invariant with respect to changes of coordinate system and has some of the properties of a distance function. Unfortunately it is lacking in just the property we want: the time interval between the two events in Newtonian space-time has the same value if we measure it along a curved path joining the two events as when we measure it along a straight path. We therefore cannot use the time intervals between events to set up a purely geometric kinematical distinction between straight and curved lines in Newtonian space-time and hence between uniform and accelerated motion. It is precisely the absolute character of Newtonian time which defeats this attempt to solve the problem of absolute acceleration by finding an independent purely kinematical distinction between accelerated and unaccelerated motion to underpin the dynamical distinction.

It seems then that the real problem of absolute space—the problem of the conflict between Newtonian dynamics and the principle of sufficient reason—could be resolved in either of two ways. Either dynamics might be revised so as to eliminate the offending dynamical distinction, or kinematics might be revised so as to legitimise the distinction at a purely kinematic level. Either we must disallow the Newtonian two spheres situation, or we must find some non-dynamical criterion for deciding which sphere is really rotating. It is a surprising historical fact that Einstein never seriously considered the second of the two alternative solutions to the problem.

Einstein seems always to have merely taken it for granted that the Special Theory of Relativity, because it preserved the traditional dynamical distinction between uniform and accelerated motion, could make no difference to the problem. He never even discussed in this context the possible relevance of the fact that, according to Special Relativity, clock measurements would provide an independent criterion for identifying accelerated systems, for settling independently of the dynamical effects which of the two spheres should be described as rotating. Einstein always simply describes the problem, says that Special Relativity makes no difference to it, and omits any mention of the possible relevance of clock measurements.

It is just possible that Einstein, at this time, did not clearly recognise that the Special Relativistic behaviour of rods and clocks was not a dynamical effect. On Lorentz's theory these would have been dynamical effects. However once we take seriously Minkowski's space-time geometrical interpretation of Einstein's kinematics it becomes clear that these effects are no more dynamical effects than is the fact that in Euclidean geometry the sum of the lengths of two sides of a triangle is always greater than the length of the remaining side. And just as this fact enables us in Euclidean geometry to define a straight line in Euclidean space as a line of shortest length, thereby distinguishing it from any curved line without having to appeal to any particular coordinate system, so in Special Relativistic space-time geometry, the analogous time-dilatation effect enables us to define a straight line in space-time, corresponding to the possible trajectory of a uniformly moving particle, as a line of maximal (proper) time interval, thereby distinguishing it from any curved line—which would correspond to an accelerated motion—without having to appeal to any particular frame of reference. It is precisely the rejection of Newtonian absolute time which makes this solution possible. But what is clear to us today may well not have been clear to Einstein in 1916.

5 THE SPECIAL RELATIVISTIC SOLUTION

We can now see that the solution to the problem of absolute space, *i.e.* of absolute acceleration, is given by Special Relativity as follows: which of the relatively moving bodies are to be described as absolutely accelerated is determined by measurements with clocks. If the path of a body is a path of maximal proper time interval then that body is described as uniformly moving, otherwise its motion is described as accelerated. This distinction can be made simply on the basis of measurements along the trajectory of that body and along neighbouring trajectories. These measurements are absolute ones whose results depend in no way on the choice of a frame of reference. We now postulate that forces, satisfying certain restrictive conditions, must be invoked to account for all departures from uniform motion, the magnitude of the force being required to be proportional to the acceleration of the body in question, *i.e.* to the (measured) extent to which its path deviates from a path of maximal proper time. There is no longer any conflict between this dynamical requirement and Einstein's epistemologically strong formulation of the principle of sufficient reason.

Thus consider again the case of the two relatively rotating spheres. Assume, for simplicity, that their gravitational fields are negligible and that they are held together by (non-gravitational) forces between their particles which are repulsive at small distances but attractive at larger distances. Now according to Special Relativity the particles of the ellipsoidal sphere, and especially those at its equator, have space-time trajectories which are not lines of maximal proper time and the curvature of these lines is proportional to the excess of attractive over repulsive forces acting on these particles which has resulted from the flattening of the sphere compared with the other sphere where all the inter-particle forces are in equilibrium. That these particle trajectories are not lines of maximal proper time may be ascertained empirically by comparing the rate of a clock attached to the ellipsoidal sphere at its equator with that of an initially adjacent clock which is stationary with respect to the other sphere. The prediction is that the clock attached to the ellipsoidal sphere will progressively lag behind the other clock when we compare their readings on subsequent occasions when they pass by each other. We predict precisely the opposite effect when we compare a clock attached to the equator of the other sphere with one stationary with respect to the ellipsoidal sphere. Both predictions can be checked empirically and without assigning a special status to any particular frame of reference. Had these tests been made prior to measuring the slight departure of either sphere from sphericity we could have predicted in advance which sphere would prove to be ellipsoidal. The principle of sufficient reason is thus fully satisfied: an independently observable difference in causal conditions is postulated to account for the difference in dynamical effects. This is the solution of the problem of absolute space which is given by the Special Theory of Relativity.

6 THE IDENTICAL GENERAL RELATIVISTIC SOLUTION

Surprisingly enough, the above paragraph is not only an accurate account of Special Relativity's widely unrecognised solution of the problem of absolute space, but I believe it to be also an accurate account of General Relativity's solution of the same problem. For Einstein's geometrisation of the gravitational field left the problem exactly where it stood as far as non-gravitational forces are concerned. Every body still has a privileged set of states of motion, namely those where it is moving along a path of maximal proper time. In so far as its trajectory deviates from such a path, non-gravitational forces must be invoked which are proportional to the amount and direction of this deviation. The principle of sufficient reason is not satisfied if we have no independent way of determining which paths are paths of maximal proper time. Gravitational theory does not provide us with an independent criterion: for example, it is clear that we must know whether or not the sun itself is moving along a geodesic (a line of maximal proper time) before gravitational theory can tell us which are the geodesic trajectories in the neighbourhood of the sun. So the General Theory's solution of the problem of absolute space is ultimately simply to fall back on the solution already provided by the Special Theory. The facts that in General Relativity the geometry of space-time is now no longer independent of the distribution of matter, that the dynamically privileged frames of reference are consequently merely local frames of reference which will not be unaccelerated relative to those in other regions, that gravitation is no longer described by forces but is built into the geometry that the General Theory and the Special Theory are in empirical disagreement as to which actual particle trajectories will prove to be lines of maximal proper time, are an irrelevance as far as the problem of absolute space is concerned. Its solution remains the one provided by the Special Theory of Relativity.

I have already showed how this solution is to be applied in detail to Einstein's problem of the two relatively rotating spheres. It turned out that clock measurements alone would enable us to predict in advance which sphere would prove to be ellipsoidal. The principle of sufficient reason is thus fully satisfied: an independently observable difference in causal conditions is postulated to account for the difference in dynamical effects.

7 THE IRRELEVANCE OF FICTITIOUS GRAVITATIONAL FORCES

Now, from what I said previously, General Relativity can, and indeed must, give the same account. But it is sometimes suggested that it gives a different account along roughly the following lines. All motions, and in particular all accelerations, are relative to the choice of frame of reference. Forces must therefore also be relative to the choice of frame of reference. In the reference system of the non-ellipsoidal sphere the other sphere is rotating and its particles therefore have accelerations varying with their distance from the axis of rotation. These accelerations are balanced by the residual non-gravitational forces generated by the ellipsoidal distortion from the equilibrium shape. However the situation is more complicated in the frame of reference of the rotating sphere. Here we must postulate additional gravitation-like forces (centrifugal forces) which increase with distance from the axis of the sphere. These precisely balance the residual non-gravitational forces, producing no resulting acceleration. These centrifugal forces also act on the other sphere, but here they are more than compensated for by oppositely acting Coriolis forces which, together with them (and the inter-particle forces, which here all cancel out), produce resultant forces precisely sufficient for the observed accelerations of the particles of this sphere, *i.e.* for the observed rotation of this sphere in our chosen frame of reference. The peculiar gravitational forces (centrifugal and Coriolis forces) required in this description are claimed to follow from a perfectly permissible solution of Einstein's field equations, namely the ordinary flat space-time solution of the source-free equations but transformed to a uniformly rotating frame of reference. Two questions now arise. First does General Relativity license us to give such a description? Second does such a description solve the problem of absolute space as Einstein and I have formulated it?

The answer to the first question is unexpectedly obscure, but the answer to the second question seems decidedly in the negative. First we cannot solve Einstein's field equations without imposing boundary conditions on the solutions; the predictive content of the General Theory must depend on how restrictive our requirements on these boundary conditions are. But the admission of the solution of the source-free equations which includes centrifugal and Coriolis effects involves relaxing some apparently natural restrictive requirements on the boundary conditions and hence, at first sight, would appear to make General Relativity an empirically weaker theory than it would otherwise be. This objection might be met by maintaining that non-covariant boundary conditions conflict with the spirit of a generally covariant theory and would not so much restrict its empirical content as arbitrarily restrict the permitted coordinate systems in which a solution of its equations could be expressed. However, if that line is taken, there then seems no clear warrant in the theory for interpreting the $g_{\mu\nu}$ in this kind of solution as reflecting the presence of a peculiar kind of gravitational force rather than as reflecting merely a peculiar choice of coordinate system. Secondly, and much more seriously, it does not seem that this proposed way of talking in fact solves Einstein's original problem. For whatever frame of reference we choose to describe the situation in, there is an observable difference between the two spheres, which the principle of sufficient reason requires us to ascribe to an independently observable difference in the causal conditions. In particular the non-gravitational forces balance everywhere on the one sphere, but not everywhere on the other, quite independently of what coordinate system we choose. But the postulated fictitious gravitational forces are not independently ascertainable: there would be no reason for picking this particular solution of the field equations prior to knowing which sphere was really an ellipsoid of revolution: unless, of course, we had already determined by means of time measurements which particles were moving along geodesics. But once we have done that we have already solved the problem, and solved it in the manner prescribed by the Special Theory of Relativity.

8 THE IRRELEVANCE OF THE NEO-MACHIAN PROGRAMME

A different tradition within General Relativity Theory attempts to reinstate Mach's view that it is acceleration relative to the rest of the universe which is the cause of the dynamical effects in question. Within this tradition it is hoped that boundary conditions can be discovered which, added to the Field Equations of General Relativity, rule out almost-empty-universe solutions and require there to be sufficient matter in the universe, for the assumption that this matter is rotating, but a small sphere is not, to have the same dynamical consequences for the sphere as would follow from the assumption that the sphere was rotating but the rest of the matter in the universe was not. Now, in this programme, either this equivalence condition is to be satisfied ***trivially***, by taking the solution of the Field Equations for which the rest of the matter in the universe is rotating to be ***identical*** with the solution for which it is not so rotating but transformed to a rotating frame of reference, or the equivalence condition is to be satisfied non-trivially, the former solution being equivalent to the latter only with respect to the dynamical effects on the small sphere. Now if the equivalence is trivially satisfied, the addition of the extra boundary conditions is pointless, for the same result can be achieved without imposing them. But if the equivalence condition is not trivially satisfied then the two solutions must differ with respect to what they say about the distribution and/or the nature of the motions of the rest of the matter in the universe. But this must then be an empirical difference and must therefore, by the principle of sufficient reason, be ascribed to some difference in causal conditions: so the problem has merely been transferred from one small sphere to the whole system of the stars. This programme is therefore caught in an apparently hopeless dilemma: ***either*** it merely shifts the problem from the absolute rotation of a small sphere to the absolute rotation of the rest of the universe—and this occurs so long as these two solutions of the Field Equations are distinct solutions with regard to what they say about the rest of the universe, ***or*** these two solutions turn out to be the same solution merely expressed in different frames of reference, in which case the programme can be trivially satisfied without any conditions on what matter there is in the rest of the universe. But I say we need not face this dilemma because the problem the programme seeks to solve has already been solved. The problem that Berkeley and Mach grappled with, but which Einstein was the first to formulate really succinctly, does not require any of these more recherché aspects of the General Theory, but has a solution which can already be precisely formulated within the Special Theory.

Nevertheless, even if everything I have said so far is logically correct, it might be argued that there is another related problem about absolute space which the Special Theory did not solve, for which General Relativity provides a partial solution, and for which the neo-Machian programme within General Relativity hopes to provide a more complete solution. This is the problem that, even in the Special Theory, space is something which 'acts but is not acted upon'; the geometry of space-time conditions the behaviour of bodies but is itself unaffected by, let alone wholly determined by, their presence. However, while this feature of Special Relativity is indeed modified by General Relativity, and may indeed have significantly influenced Einstein in his construction of the General Theory, I cannot see in it any cogent philosophical objection to absolute space. The difficulty is that to turn it into any cogent philosophical objection to a theory, rather than a mere physical hunch which has in fact proved partially successful, one must formulate some more general physical or philosophical principle which nature conforms to in all, or nearly all, other cases, and of which this objection to absolute space in the Special Theory is merely a particular instance. Now the principle in question clearly cannot be merely that of equality of action and reaction, for that is inconsistent with the relativity of simultaneity, but is, in an extended sense, already satisfied by Special Relativistic dynamics in the form of overall momentum conservation. The principle in question must be some more general principle of cause-effect *reciprocity*. But I cannot find any such principle which does not have one or more of the following unfortunate consequences: the geometry of space-time determines the motions of bodies irrespective of their masses, therefore the motions of bodies irrespective of their masses ought to determine the geometry of space-time. The charges of bodies influence their motions, therefore their motions ought to influence their charges. More generally, any interaction which depends on certain attributes of bodies could not conserve those attributes without apparently violating a general principle of cause–effect reciprocity. I conclude that no plausible general principle of such a kind can be formulated and hence that there is no cogent philosophical objection to absolute space implicit in this particular criticism of Newtonian and Special Relativistic physics. The only entirely cogent philosophical criticism of absolute space which I have been able to unearth is indeed the objection which Einstein gave most prominence to, and which is satisfactorily answered, surprisingly enough, by the Special Theory of Relativity.

Nevertheless, comments I have received from two eminent relativists on an earlier draft of this paper have convinced me that there is a sound philosophical justification for part of the neo-Machian programme. For there is one respect in which orthodox General Relativity is not even as satisfactory as ordinary Special Relativity in the account it gives of these matters. Namely, the field equations fail to determine the geometry even to the extent of failing to determine the curvature uniquely. Even in the absence of matter both flat and curved space are consistent with the equations, which only require that the contracted curvature tensor, rather than the full curvature tensor, be fixed everywhere by the distribution of matter. In principle this defect of the orthodox theory can lead to situations at least as paradoxical as the Newtonian two spheres situation, and it certainly seems correct to seek for some way of supplementing the usual equations so as to remedy this defect, and to render the General Theory's solution of the problem as satisfactory as that of the Special Theory.

However some neo-Machians (*e.g.* Raine [1975]) wish to go further and to require conditions which ensure that a knowledge of the curvature everywhere should uniquely determine the metric everywhere and hence which lines are and are not geodesics. It is true that the space-time geometry of Special Relativity does not satisfy such a condition. Given two specified paths in space-time I cannot say, *a priori*, which, if either, will prove to be a geodesic. But if this is an objection to the space-time of special relativity, it is equally an objection to the space of ordinary Euclidean geometry. The curvature (zero everywhere) does not in that case determine, in advance, which of two prescribed lines will prove to be the straight one, *i.e.* the one of shortest length. If there is an *a priori* philosophical objection to Special Relativity here, it is equally an *a priori* philosophical objection to ordinary Euclidean geometry. More argument would be required to convince me of such a philosophical *tour de force* than the neo-Machians have yet offered.

LITERATURE

I have tried to keep the arguments of this paper self-contained, so have avoided references to the extensive technical literature on attempts to incorporate some version of Mach's Principle into the General Theory. Criticisms of the orthodox view that General Relativity solved the problem of absolute space are to be found in Fock [1959] and Grünbaum [1964; 1973], and also in many recent texts on General Relativity. A recent discussion is Grünbaum [1976]. Views more or less diametrically opposed to those of the present paper are to be found in Sciama [1959], [1972].

Chelsea College, London

REFERENCES

Fock, V. [1959]: *The Theory of Space, Time, and Gravitation*. New York: Pergamon Press.

Grünbaum, A. [1964; 1973]: *Philosophical Problems of Space and Time*. London: Routledge; 2nd edn. Boston and Dordrecht: Reidel.

GRÜNBAUM, A. [1976]: 'Absolute and relational theories of space and time', *Minnesota Studies in the Philosophy of Science*, **8.**
RAINE, D. J. [1975]: 'Mach's principle in General Relativity', *Monthly Notices of the Royal Astronomical Society*, 171, p. 507.
SCIAMA, D. W. [1959]: *The Unity of the Universe*. London: Faber.
SCIAMA, D. W. [1972]: *The Physical Foundations of General Relativity*. London: Heinemann.

Brit. J. Phil. Sci. 29 (1978) 325–346 *Printed in Great Britain*

Carnap and de Finetti on Bets and the Probability of Singular Events: The Dutch Book Argument Reconsidered*

by KLAUS HEILIG

Introduction
1 *Fair betting quotients and inductive probability in Carnap I*
2 *Coherent betting quotients and subjective probability in de Finetti*
3 *Real betting quotients and the economics of bookmaking*
4 *Rational betting quotients are not probabilities at all*
5 *Strictly coherent betting quotients in Carnap II*
Conclusion

INTRODUCTION

Recent contributions by Baillie and Ellis[1] will have made readers of this *Journal* familiar with the so-called 'Dutch Book Argument'. This provides the rationality constraints for a widely accepted procedure of numerically measuring subjective degrees of belief in the occurrence of singular events by means of personal betting quotients in such a way that they can be taken as a concrete application of the abstract calculus of probability. The basic term here—probability—is defined by several axioms which state, roughly speaking, that the probabilities of mutually exclusive events are real numbers in the range from 0 to 1, both bounds included, which sum up to 1. A betting quotient q is the ratio of the sum hazarded (here called the 'stake') by the individual in question on the occurrence of the event E to the sum S (here called the 'total stake' or the 'gross gain') which will be won if E occurs. The more the individual believes in the occurrence of E, the more he will be prepared to risk for a particular sum S. Hence the betting quotient increases with degree of belief. Since the minimal degree of belief will result in risking nothing, and since the maximal degree will lead to a sum hazarded which does not exceed the expected gross gain, betting quotients satisfy by definition the first axiom of the calculus of probability. And it has been argued that the Dutch Book

Received 10 *September* 1977

---

* Research on which this essay is based has been supported by the Thyssen-Foundation in Cologne.

[1] Baillie [1973] and Ellis [1973].

argument shows that the second axiom, called the 'sum condition' by Vickers,[1] is also satisfied: the betting quotients must satisfy the sum condition, for if not a very plausible minimal premise of economic rationality will be violated. This premise is encapsulated in the motto: do not throw money out of the window. Betting quotients and the Dutch Book argument are connected with bookmaking as, for example, it is pursued professionally in Great Britain in connection with horse racing. In this case the motto for our individual would be: do not bet in such a manner that it is already clear before the race that a loss will result. The term 'Dutch Book' was adopted from bookmakers' parlance by Lehmann in 1955 to describe such an unfavourable situation.[2] The Dutch Book argument states conversely, then, that a Dutch Book against the individual is not possible if and only if the betting quotients fulfil the sum condition. Since it would be unreasonable to allow such a Dutch Book against oneself, rationality requires that at the very least one should only declare such betting quotients which fulfil the sum condition. Subjective degrees of belief measured by individual betting quotients could, therefore, indeed be understood as probabilities in the sense of the probability calculus.

My thesis is not that this argument is invalid or false in a formal or logical sense, but that because of its very restrictive *conditions of validity* it is neither of empirical significance nor should it be normative. In other words, neither *can* all conditions be empirically fulfilled simultaneously, nor would it be reasonable to demand that they *should* be fulfilled by rational individuals. Hence it is completely illusory to attempt to prove, with the help of betting quotients, that subjective degrees of belief in the occurrence of singular events fulfil the axioms of the probability calculus.

This thesis follows from a comparison of the main points of the essay in which the Dutch Book argument was presented in full detail for the first time, de Finetti's [1931], with the practice of British bookmakers. Never having visited a race course nor a bookmaker, my knowledge of the latter is restricted to several published sources, whose arguments, however, appear to be correct. Ramsey had already briefly hinted at the Dutch Book argument in his critique of the Keynesian 'degree of partial belief' in 1926.[3] Nevertheless, these isolated comments, which were posthumously published in 1931, cannot be considered as the point of departure of the Dutch Book argument. Rather it was first developed at length by de Finetti in his [1931]. This article will thus be the starting point of my essay: in it, all of the conditions of the validity of the Dutch Book argument are presented (if not discussed) in detail, and in a far clearer fashion than in any of his many later publications, certainly clearer than in his frequently cited article of 1937.[1] The same holds true for the presentations of de Finetti's theory by other authors with which I am familiar. To be sure, de Finetti did not speak of Dutch Books, but rather of 'coherent' betting quotients. Nevertheless, betting quotients are coherent when they do not allow for a Dutch Book, and they do not allow for one when they fulfil the sum condition. So, 'Coherence-theorem' and 'Dutch Book argument' are two different names for the same thing.

---

[1] Vickers [1976], specifically chapter 4: 'Coherence and the sum condition', pp. 78–95.

[2] Lehmann [1955], p. 251. Unfortunately, I have not succeeded in discovering the origin of that tradition of British (or American?) bookmakers (mentioned by Lehmann) of designating such a foolish business practice as 'Dutch'.

[3] See Ramsey [1931]. He writes: 'Having degrees of belief obeying the laws of probability implies a further measure of consistency, namely, such a consistency between the odds acceptable on different propositions as shall prevent a book being made against you' (p. 81), 'by a cunning bettor', which would mean that you 'would then stand to lose in any event' (p. 80).

Lehmann's article and two others by Kemeny and Shimony[2] all reply to Rudolf Carnap's attempt in his [1950] to introduce the concept of inductive or logical probability$_1$ (a quantitative concept completely separated from that of statistical probability$_2$) as a degree of confirmation.[3] This was done with the help of *fair* betting quotients. These fair betting quotients did not originally have anything to do with de Finetti's coherent betting quotients. But Lehmann, Kemeny and Shimony attempted to provide Carnap's original idea with a different foundation by applying axioms for degrees of confirmation based on the 1937 version of de Finetti's coherence-theorem. Carnap[4] himself accepted this suggestion and began (before 1961) to give up his original attempt in favour of de Finetti's theorem. Stegmüller, who has argued strongly for Carnap's ideas, introduced a distinction between Carnap I and Carnap II as a result of other evident modifications in his positions.[5] I shall adopt this convention. Although Carnap II no longer supports the arguments of Carnap I which were connected with fair betting quotients, he still speaks of them. Furthermore Carnap II continues to uphold several of the consequences of the original exposition, consequences which have affected the reception of de Finetti's theorem by other authors. For the sake of clarity, therefore, it would be helpful to first investigate briefly fair betting quotients and inductive probabilities in Carnap I, and to offer some reasons for the transition to coherent betting quotients (1). The latter, together with de Finetti's subjective probabilities, will then be considered in detail (2). A comparison with the economic behaviour of bookmakers will then be undertaken in 3. The reasons for the failure of the Dutch Book argument will then become evident: rational betting quotients are not probabilities at all. This is pointed out in 4. In 5 I show that even the sharpening of the concept of coherence to strict coherence by Shimony and Carnap II does not affect this conclusion.

---

[1] de Finetti [1937].

[2] Kemeny [1955] and Shimony [1955].

[3] Carnap [1950], Preface, p. viii and *passim*.

[4] See Carnap and Jeffrey [1971]. Introduction, p. 2.

[5] Stegmüller [1973], where the new theory of Carnap II is presented and discussed in detail.

I FAIR BETTING QUOTIENTS AND INDUCTIVE PROBABILITY IN CARNAP I

In his *Logical Foundations of Probability* Carnap attempts to distinguish sharply between probability$_2$, a relative frequency in the statistical sense, and the logical or inductive probability$_1$ which, as a degree of confirmation, indicates how strongly an hypothesis is confirmed by the experimental data. This degree of confirmation is measured by the betting quotient which indicates those conditions under which an individual who has the experimental data at his disposal is willing to bet on the correctness of the hypothesis. These betting quotients should be *fair*, i.e., they should not favour either partner. Whether the individual considers a quotient to be fair or not can be ascertained by whether or not he would be willing to exchange roles. Technically, this would mean that he would be equally willing to bet *on* the hypothesis with the betting quotient q, as he would be to bet *against* the hypothesis and consequently *on* the anti-hypothesis, with the betting quotient $(1-q)$.[1]

One can, in general, easily ascertain which betting partner has bet on an hypothesis or an event and which against it by noting which of the two achieves a (positive) net gain when the hypothesis proves to be correct or when the event occurs. In general, the following pattern is yielded:

Table I

	Net result of the bet on E	 against E
E occurs	$S-qS=(1-q)S$	$-S+qS=-(1-q)S$
E does not occur	$-qS$	$+qS$

Given positive values of S, which we can presuppose here with Carnap I, it can be established that that person who achieves a positive net gain when the event occurs bets *on* the event and that person who achieves a positive net gain when it fails to occur bets *against* the event. These characteristics could be called the *direction* of the bet.

[1] See Carnap [1950], p. 164.

Thus, objective criteria not subjective notions are decisive in determining the fairness of betting quotients. However, in his attempt to explicate these objective criteria Carnap falls back into the conceptual world of statistical probability$_2$. Although he assumes that betting on *singular* hypotheses is 'the most important kind of betting situation',[1] he nevertheless determines the fair betting quotient for the hypothesis that an object *b* exhibits the characteristic *M* as the relative frequency of objects possessing the characteristic in question within a reference class of otherwise similar objects. The degree of confirmation of the supposedly singular hypothesis is then interpreted as the degree of confirmation of an hypothesis which is *repeatable* under the same conditions. And this latter is itself explained by the relative frequency of objects possessing the characteristic in question. Instead of the elliptical formulation: 'The probability$_1$ that the object *b* possesses the characteristic *M* amounts to $\frac{1}{5}$', one could say equivalently, but more clearly: 'The probability$_2$ that objects of the sort *b* exhibit the characteristic *M* amounts to $\frac{1}{5}$.' Hence, every probability$_1$-proposition could be replaced by a probability$_2$-proposition.

It seems clear, then, that the concept of fairness in Carnap I is identical with the concept of a fair lottery, where the weighted arithmetical average of the prizes, normally known as 'mathematical expectation', is equal to the fair price of a ticket. If the relative frequency within the reference class of objects with the characteristic *M* amounts to $\frac{1}{5}$, so that the fair betting quotient also equals $\frac{1}{5}$, then in a situation of *n* bets with this betting quotient where *n* times 1 pound is staked, in exactly $\frac{1}{5}$ of all bets 5 pounds will be won, and in $\frac{4}{5}$ of all bets 1 pound will be lost. The sum of the stakes equals the sum of the winnings. The betting is fair in the well-known sense that with the conclusion of all bets none of the participants gains or loses, so that each could quite happily take the role of the other. Fairness in a game of chance, and hence also with Carnap's fair betting quotients, is a requirement which is only defined for probability$_2$ and only for 'very many' or all bets. For there is no rational argument which could explain why anyone, in using a betting quotient which is fair in the long run, should be prepared to reverse his bet, to change its direction, in the case of a single bet. Plausible counter-examples are provided by extreme betting quotients. In a situation where one could bet 1 pound only once on an event whose probability$_2$ amounted to 1/1000, one could, with a fair betting quotient of 1/1000, attain a net win of 999 pounds if the event actually occurred and otherwise would lose 1 pound. Probably no one in this situation would be willing to exchange roles with his opponent who can lose 999 pounds but only win 1 pound.

[1] *Op. cit.*, p. 167.

To show as clearly as possible the difference between this and de Finetti's coherent betting quotients, it should be mentioned that, whereas according to Carnap I one can speak of the fairness of a single betting quotient, for de Finetti only all betting quotients together can be coherent or not coherent. Furthermore, the problem of the sum condition does not exist for Carnap I, since probability$_1$ can always be explained by probability$_2$, that is by a concept which no one would deny fulfils the axioms of the calculus of probability. This reversion had already been clearly established by von Mises in 1951:

> I do not see why one cannot admit to begin with that any numerical statements about a probability$_1$, about plausibility, degree of confirmation, etc., are actually statements about relative frequencies.[1]

Carnap's bets are not bets in the proper sense, *i.e.*, contracts which are *not* governed by the laws of chance,[2] but rather they are just as much games of chance as are lotteries.

2 COHERENT BETTING QUOTIENTS AND SUBJECTIVE PROBABILITY IN DE FINETTI

Whereas in his [1950], and even in the second edition of 1962, probability was primarily intended to be applied to inductive logic, Carnap's interests subsequently shifted in the direction of rational decision theory.[3] This was developed in economics for the purpose of rationalizing non-repeatable decisions and therefore its central maxim of the maximization of expected utility essentially deals with the application of a subjective concept of the probability of singular events. Consequently Carnap no longer pursues his previous argument involving fair betting quotients, presumably because he recognised the obvious weaknesses of his original theory. Rather he adopts de Finetti's theory of subjective probability together with his coherence-theorem.

In de Finetti, there is no implicit connection with probability$_2$. Rather subjective probabilities relate to single events such as horse races which are not repeatable under the same external conditions. His attempt to measure a subjective degree of belief (*grado di fiducia*) is, in terms of his original claim, an empirical, descriptive or operational one. Subjective probabilities must also, in principle, be measurable by means of betting quotients which arise empirically.[1] But the results of empirical research have apparently not agreed with the theory very well, for, in a new footnote to the English translation, de Finetti shifts to the normative level (which is the only level upon which Carnap II argues) when he writes that (his) 'probability theory is not an attempt to describe actual behaviour; its subject is coherent behaviour, and the fact that people are only more or less coherent is inessential.'[2]

---

[1] von Mises [1957], p. 97. Within the framework of recent attempts to explicate probability$_2$ by means of propensities to produce relative frequencies rather than by relative frequencies themselves, Hacking ([1973], p. 485) comes to the same conclusion: 'I shall maintain that all degrees of confirmation, P_1, depend on statements of propensity using P_2.' Stegmüller [1973] levels a supposedly 'lethal' objection against the work of von Mises, which I attempted to neutralise in my [1975] article by reviving the frequency theory using an antidote. This article also contains an initial argument against de Finetti, which however, as I have since discovered, is not correct.

[2] Sidney [1976], p. 3.

[3] This occurred at the latest in 1965 with the essay 'Inductive Logic and Inductive Intuition'; see Carnap [1968], p. 260.

The three validity conditions of de Finetti's coherence-theorem can be most simply understood by way of extracts from the original. I have indicated which condition is being described by inserting the relevant numbers in parentheses following the pertinent passages.

> In order to measure the degree of belief that a given subject O is supposed to have in the occurrence of an event E, we must presume . . . that he could be obliged to hold a bank of bets (1) for or against (3) a certain number of events which would include event E among them. . . . The conditions of the bet are determined in the following manner: It is the task of subject O, who holds the bank, to determine the price p of a voucher or bond(1) which entitles one to receive one Lira when a particular event E occurs. This means that he is obliged to buy or sell as many vouchers at the given price(3) as the public wishes(2). If a bettor appears at O's bank and wishes to bet on the event E, he buys a voucher at the price of pS which gives him the right to demand a particular sum S if he wins the bet, i.e., if E occurs. (Conversely, if he wishes to bet against the event he can commit himself, in exchange for the payment of an amount pS from his opponent, to pay the sum S if he loses the bet, i.e., if E occurs. This case is transformed into the previous one if we consider negative values of S there (3).)

and:

> The evaluation of the probability of the event E with p signifies that the one who holds the bet-bank(1) declares himself prepared to accept every bet with any bettor who can fix the gross gain S as he wishes(2), whether positively or negatively(3).[3]

We can, accordingly, tentatively formulate the three conditions for determining whether an individual's evaluation of probabilities is coherent or non-coherent as follows. The individual must state that he is prepared:

(Condition 1) to strike bets on all events at the betting quotients which he himself fixed;

(Condition 2) to allow the opponent to fix the amounts of the gross gains and thereby the stakes; and

---

[1] See de Finetti [1937], p. 150.

[2] *Ibid.*, p. 111. See also Carnap [1971*a*], p. 13.

[3] de Finetti [1931], p. 304 and p. 308,

(Condition 3) to allow the opponent to determine the sign of the gross gains according to his wishes.

Disregarding condition 3 for the moment, it is apparent that the first two conditions of the coherence-theorem do not describe the behaviour of a normal race goer, but rather that of a professional bookmaker. The coherence-theorem, then, can only be valid for bookmakers who, as is well-known, declare themselves willing to accept any amount as stakes, under conditions which they themselves have fixed. Thus when Hacking, in his account of the matter, writes, 'we dispense with the irrelevant custom of leaving our money in the hands of a bookmaker or arbiter until the issue is settled', since this custom 'is due to human dishonesty and has nothing essential to do with betting',[1] he misunderstands the role which bookmakers play in connection with the Dutch Book argument. It may be sensible in private betting to engage a third person as referee who arbitrates in cases of doubt or conflict and pays out the total stake to the winner. In professional betting, however, the bookmakers are *not* arbiters of the bets of others but are betting partners themselves, inasmuch as the result of the race and, accordingly, of the bet is established by others, namely, the organisers of the race (the Jockey Club).

Since the coherence-theorem can only be valid for bookmakers, it seems reasonable to inquire into the principles according to which they conduct their business, in particular, whether or not they fix their betting quotients so that they fulfil the sum condition.

3 REAL BETTING QUOTIENTS AND THE ECONOMICS OF BOOKMAKING

First of all, it is important to make clear the difference between totalisator-betting and betting at fixed odds. In totalisator-betting the win dividends are established after the race by dividing the total stakes among those bettors who bet on the winning horse. The bettors bet among and against each other, which is why this sort of betting is called *pari mutuel* in France, where it appears to have been developed.[2] Hence, the organiser of the betting takes no risks since he has no liabilities. On the contrary, if the rules of the game allow him to keep a certain percentage of the sums paid in he does a safe business. He can, without hesitation, accept any amount as a stake on any horse.

With betting at fixed odds on the other hand the bookmaker commits himself before the race to paying his opponents a sum if a particular horse wins, a sum which is calculated by means of the betting quotient from the opponents individual stake. This form of betting, unusual in Germany, is the dominant form in Great Britain. It is evident that as opposed to mechanical totalisator-betting, betting at fixed odds demands the skilful consideration of certain economic factors if the bookmaker is to make a living.

---

[1] Hacking [1968], p. 284 and [1967], p. 316.

[2] See p. 1103 of vol. 8, of the 15th edition of the *Encyclopaedia Britannica* (article on "Wagering", pp. 1103–5).

British bookmakers do not usually give the conditions under which they wish to make bets in the form of betting quotients, but rather in the form of 'odds *against* a particular horse winning a race',[1] which are also known as 'prices'.[2] These indicate the relation between the bookmaker's stake and the opponent's stake, amounting to the total stake S_i, and hence, can be written and dealt with as fractions. Odds of '4 to 1' signify, then, that for every money unit of the opponent's stake which is bet *on* the victory of a particular horse, the bookmaker adds another four money units and pays out a total of five (the take-out) if the particular horse chosen wins the race. A betting quotient q is therefore related to the corresponding price p as follows:

$$q = \frac{1}{p+1}, \quad \text{and conversely} \quad p = \frac{1}{q} - 1$$

The bookmaker always bets *against* a horse, while the opponent always bets on it. The bookmaker says 'this horse will not win'; the opponent, called the backer or the punter in standard racing terminology, says 'it will win'. Bookmakers offer prices for all horses and accordingly offer bets of any magnitude wished (below particular limits which are determined by their financial reserves). They thereby fulfil conditions 1 and 2 of the coherence-theorem; not, however, condition 3, since they only accept positive S_i.

What do they do, however, in order to secure themselves against a Dutch Book? For bookmakers must above all avoid a Dutch Book against themselves, a situation which they call 'over-broke', which is 'worse than just being broke'.[3] This danger is always present if the sum of the betting quotients is smaller than 1. An opponent can then make a system of bets by means of a skilful combination of stakes on all horses which guarantees him a gain. He can most simply calculate how much he must hazard for each bet if he undertakes to achieve a gross gain S of the same magnitude in every single bet. This amount is determined, when only whole

[1] Chenery [1963], p. 25.

[2] Sidney [1976], p. 4. It is from this book, particularly chapter 6, pp. 85–106, that I have learned most about bookmaking. For the backer's point of view, Mitchell [1972], chapters 1 and 2, pp. 1–43, is a valuable source.

[3] Sidney [1976], p. 96.

money units are used, as a (lowest) common multiple either of the sums of the numerators and denominators of the odds (prices) or of the reciprocals of the betting quotients. If, for example, the prices (odds) are 4/1, 3/1 and 1/1, and therefore the corresponding betting quotients 1/5, 1/4 and 1/2, then 20 is the lowest common multiple. Consequently the opponent must stake 4 on the first horse, 5 on the second, and 10 on the third, a total of 19, in order to win a gross gain of 20, independently of the outcome of the race, and, therefore, to be certain of a total gain of 1.

In order to prevent such a Dutch Book (the over-broke-stake) the bookmaker will raise the sum of the betting quotients to 1 or to more than 1. That a Dutch Book against the bookmaker is impossible in either of these cases can be proved as follows.

Presuppositions

(1) The opponent must bet on all events. Otherwise a Dutch Book is impossible already, independently of the sum of the betting quotients, since the opponent must suffer a loss if an event occurs on which he had not bet. Hence, all stakes q_iS_i must be > 0, and consequently, since the betting quotients q_i cannot by definition be < 0, all q_i and all S_i must be > 0.

(2) The sum of the stakes $\Sigma q_iS_i = C$.

(3) If the horse i wins the total gain $G_i = S_i - C$.

Proposition

If $\Sigma q_i > 1$, not all G_i can be > 0.

Proof

Assume, on the contrary, that all $G_i > 0$, then it follows that all $q_iG_i > 0$ and that $\Sigma\, q_iG_i > 0$. Using (3), one gets $\Sigma\, q_i(S_i - C) > 0$ or $\Sigma\, q_iS_i - \Sigma\, q_iC > 0$. By (2), $C - \Sigma\, q_iC > 0$ and hence $\Sigma\, q_i < 1$. This means, however, that all G_i can be > 0, only if $\Sigma\, q_i < 1$. Conversely not all G_i can be > 0 if $\Sigma\, q_i \geqslant 1$. Q.E.D.

This theorem, which underlies the usual practice of bookmakers, guarantees that they adhere to the premise of rationality mentioned in the *Introduction* above. Because this states only negatively what should be avoided and not what should be done positively, I shall term it *a negative* rationality premise for the economic behaviour of bookmakers.[1] Furthermore, because further premises will emerge in the course of the exposition, I shall term the above the *first negative rationality premise.*

[1] The expression 'rationality premise' comes from Baillie [1973], p. 393.

If actual bookmakers can avoid a Dutch Book by means of betting quotients which fulfil the sum condition as well of those which add up to more than one, the question arises as to which of these alternatives they will choose. Certainly every bookmaker, as a businessman, acts with the aim of making a gain through his activity. This *positive rationality premise* is the most general economic principle for any professional business activity and hence in particular for bookmaking. Consequently, all bookmakers together will strive to pay out less in winnings than they had collected from all of the opponents. As with the totalisator, they therefore retain a certain percentage of the stakes. They do not do this, however, by simply subtracting their own profit from the sum of the stakes and paying out the rest; they have, after all, made bets at fixed odds. Rather, they determine the odds from the beginning so that if all bets are made sufficiently often this will be the result. The group of all bookmakers will, however, only achieve this goal if every individual strives for it. Consequently, every bookmaker fixes his betting quotients in such a fashion that they add up to more than one, usually to 118 per cent.[1] The excess percentage represents the bookmaker's margin of gain. For a single race, this is only a potential gain, because he cannot be certain of having set the right relation on all horses. But over the years, a margin of this order is realised; 10 to 20 per cent 'is probably a good indicator both of bookmakers' gross margins and what backers collectively can expect to lose in the long term'.[2] If a bookmaker is lucky he can already be certain of a gain before the race, just as the opponent can if, conversely, the sum of betting quotients is less than 1. Whereas, to be sure, the clever opponent cannot be prevented from enjoying the benefits of the positive effect of a Dutch Book when the sum of the betting quotients is favourable for him, the bookmaker really must have luck since he has no direct influence on how much is staked on what. Of course, by changing the betting quotients, he can try to make his customers strike just those bets which he still needs in order to 'bet over'. This agreeable state of affairs which is the ideal position for any bookmaker,[3] was known in the days of the pioneers of British race course betting as 'making a book' or 'round betting'.[4] In this way we have also uncovered the proper meaning of the term 'bookmaking'.

If the bookmaker, however, were to fix his betting quotients in accordance with the sum condition, he would just be a pure 'stakeholder' in Hacking's sense. Like the totalisator, he would only collect the bets of the backers and mediate them against one another, without having a fixed percentage or even retaining anything in the long run. Therefore, although he would fulfil the *first negative rationality premise* he would *not* fulfil the *positive* one. Certainly under these conditions no-one would make bookmaking his profession, since it would be impossible to earn anything.

---

[1] According to Sidney [1976], p. 96 and to information I received by telephone from a specialist in a large firm of British bookmakers.

[2] Mitchell [1972], p. 13.

[3] *Ibid.*, p. 7.

[4] Sidney [1976], p. 96.

In the practical business of the bookmaker, which takes place at a race course with little more than fifteen minutes between two races,[1] the initial betting quotients are only specified bids for betting with which to open the market. In this market, in which many backers and several bookmakers participate, the bookmaker constantly changes his prices, for two reasons.

In the first place, the various bookmakers offer bets on the same horse at initially varying prices. So long as a bookmaker is interested in concluding bets on a particular horse, he must adjust his price to that of the other bookmakers, since the bettors will give preference to longer odds and consequently they will strike their bets with the bookmaker who offers them the best terms.

In addition to these *external* reasons for price changes, there are also some important *internal* reasons. If, after the betting market opens, the bookmaker assumes high liabilities by accepting several bets on the horse X with the longest odds so that the amount to be payed out is very high in relation to the stakes paid in by the backers, he must then attempt to lessen his risk. He does this by diverting as much money as possible onto other horses with up to this point shorter odds. The liability arising from the bets on horse X (in other words, the amount which the bookmaker would have to pay from the cash which he brought to the races to cover the difference between the sum taken in as stakes and that which would have to be paid out at that stage if X won) can only be diminished if more money is played on horses other than X. So, to control his liability, the bookmaker will reduce the price for X from, *e.g.*, 10/1 to 5/1 and lengthen that for the other horses. The new betting quotients must, of course, also add up to at least one in order to exclude a Dutch Book.

Having sketched the basics of bookmaking, I now want to examine more closely the opponent, whom Ramsey called the 'cunning bettor', a figure essential for de Finetti's argument.[2] The 'cunning bettor' does not, at first glance, conform to the picture which one has of a normal bettor who bets on the victory of *one* horse, but not on the victory of each horse simultaneously. Our opponent has probably no chance to make a Dutch Book against *one* bookmaker at *one* point in time, since no bookmaker would be stupid enough to offer betting quotients with a sum of less than one. This would simply be throwing money out of the window. However, when one considers the changes in prices given by the various bookmakers, it becomes apparent that the system bettor can succeed in betting over in two ways.

---

[1] Mitchell [1972], p. 34.

[2] In the meantime, de Finetti has suggested a second procedure supposedly equivalent to the first. Thus the consideration of the older and generally accepted argument does not lose its significance; *cf.* de Finetti [1974], pp. 83–9, and Gillies [1972], pp. 140–1.

If the prices are different from bookmaker to bookmaker at one point in time, he might be able to bet on, say, three horses with one bookmaker and on the rest with the other bookmakers in such a way that the betting quotients of *his* bets add up to less than one. In this case, he would have made a Dutch Book in his favour, if not against one, then against several bookmakers. Mitchell[1] gives an example of such a *static* Dutch Book, made against several bookmakers at one point in time (though the example is atypical for horse race betting since it deals with a match, a race of only two competitors). For the 1971 Boat Race, two bookmakers offered bets according to the following conditions:

Table II

Bookmaker	Cambridge	Oxford	Sum
A	$p = 2/5$, $q = 5/7 = 0.71$	$p = 2/1$, $q = 1/3 = 0{\cdot}33$	1·04
B	$p = 1/6$, $q = 6/7 = 0.86$	$p = 5/1$, $q = 1/6 = 0.17$	1·03

Although each bookmaker could avoid a Dutch Book against himself, the skilful opponent could, for example, stake 30 on Cambridge with *A*, and 7 on Oxford with *B*. He would then win something in either case since his betting quotients (0.71 and 0.17) add up to less than one. His stakes then amount to 37, whereas the gross gain in either case would amount to 42.

But an opponent can also complete a Dutch Book, a *dynamic* one, against *one* bookmaker if he initially bets on several horses and then bets on the remainder afterwards, if the bookmaker is forced by internal reasons to change his prices. Such a case has been indicated by Sidney[2]: a financially strong opponent bets a great deal at the beginning on the horse with the longest odds, that is with the smallest betting quotients. This potential liability forces the bookmaker to shorten these odds and correspondingly lengthen those on the other horses. Since longer odds mean smaller betting quotients, the opponent can again attain a Dutch Book by now betting on the remaining horses if the sum of the betting quotients of *his* bets is less than one. It is precisely because the bookmaker wants to avoid

[1] Mitchell [1972], p. 37–8.

[2] Sidney [1976], p. 97.

a static Dutch Book that he must allow a dynamic one, and there is no protection against such a risk.

Thus, the opponent who simultaneously bets on all horses, at first glance a strange figure, turns out to be the most dangerous backer, not only for the single bookmaker, but for all bookmakers together since he tries systematically to take advantage of the inconsistencies that inevitably occur in the hectic routine of the bookmaking business.

4 RATIONAL BETTING QUOTIENTS ARE NOT PROBABILITIES AT ALL

From the first two conditions of validity of the coherence-theorem it followed that it can only be meaningful for professional bookmakers. However, since the betting quotients of actual bookmakers total more than one, the successful practice of the British bookmakers would stand in opposition to the Dutch Book argument, if it were not for the third condition underlying it. The opponent cannot only freely determine the amount, but also the *sign* of the gross gain, and thereby of the stakes. This means, however, that he can choose the *direction* of the betting, which becomes clear by considering the following. In Table I on p. 328, the first column with positive values of S is the result column of the bettor who bets *on* an event. By changing the positive values of S to negative ones, the first column is transformed into the second, which represents the results of the bet *against* the event. Thus, $(1-q)S$ becomes $(1-q)(-S)$ and hence $-(1-q)S$, and $-qS$ becomes $(-q)$ $(-S)$ and thus $+qS$. (This is in effect how it is expressed by de Finetti: see the last sentence of the first quotation on page 331 above.)

It is now easy to see which choice an opponent would make in order to attain a Dutch Book to his advantage; of course, he would always choose stakes with the same signs for all bets, since otherwise at least one G_i would have a different sign from the remainder and this would immediately exclude a Dutch Book:

If the individual fixes the betting quotients in such a manner that their sum is *smaller* than one, the opponent would choose *positive* gross gains S_i, and thereby positive stakes q_iS_i. The individual then assumes the role of a bookmaker, betting *against* the events and foolishly giving his opponent the opportunity to make a Dutch Book.

If, however, the individual fixes the betting quotients in such a manner that their sums are *greater* than one, then the opponent would choose *negative* gross gains S_i, and thereby negative stakes q_iS_i. The individual then bets *on* the events as does an actual opponent, but can be forced by his opponent to place bets on all the events in such a way that the opponent assures himself of a gross gain.

The following example should demonstrate the case concerning negative stakes. Given betting quotients of 1/2, 1/3 and 1/4, the sum of which is

$$\frac{6+4+3}{12}=\frac{13}{12},$$

then the betting opponent would choose stakes of -6, -4, and -3, totalling -13, in order to 'receive' a 'win' of -12 in any case. He is then assured of a gross gain of $G_{1/2/3}=-12-(-13)=+1$.

In practice this means as de Finetti made clear in the first passage quoted above (p. 331) that the opponent forces the individual to place bets of 6, 4, and 3 *on* the various horses with him. He himself, however, must pay out only 12. From this we must draw the same conclusion that de Finetti does: if an individual fulfils ***all three*** conditions, he excludes the possibility of a Dutch Book against himself if and only if the betting quotients fulfil the sum condition.

Hence the coherence-theorem and its explicit conditions can now be summarized as follows.

If an individual is willing to:

(Condition 1) make bets on all events at betting quotients that he himself has fixed;

(Condition 2) have the amounts of the gross gains and the stakes

(Condition 3) as well as their signs, and thereby the direction of the betting, arbitrarily fixed by the opponent,

then a Dutch Book against the individual is excluded only if the sum of the betting quotients is exactly one,

because otherwise the opponent can make a Dutch Book against the individual. If the sum of the betting quotients is *less* than one, he can do this through ***positive*** stakes; if it is ***more*** than one, through ***negative*** stakes.

Since the individual is obliged by these three conditions to accept every bet, Shimony's and Baillie's objections which suggest that bets could be rejected do not apply. Thus Shimony has written:

> In spite of the elegance of de Finetti's theorem, however, the argument in toto has a loophole: a person may be unwilling to have a Dutch book made against him and yet willing to forego the automatic protection against this contingency which laying bets in conformity with the laws of probability provides; in other words, he may be willing to pay the price of vigilance against certain wily proposals of betting combinations in order to preserve his freedom to arrange a betting strategy with other purposes in mind.

And, Baillie similarly writes:

> We would have to suppose that our partner would attempt to take advantage of us by setting stakes for which he was bound to win, and further, that we would be silly enough to accept.[1]

My own criticism of the coherence-theorem is now easily formulated. If an individual fulfils the sum condition by satisfying the conditions of the theorem, then he certainly avoids a Dutch Book and hence fulfils the first negative rationality premise. However, he also violates the positive one, since in the long run he can attain no wins and he becomes merely a 'stakeholder', receiving nothing in return for his efforts. It is therefore most unreasonable to expect all conditions to be fulfilled. It follows that no rational person would fulfil all of them and that it would also be unreasonable to demand normatively their fulfilment from a rational person. Under the conditions of de Finetti's theorem, rational betting quotients are not possible in the sense of the negative as well as of the more comprehensive positive rationality premise. Conversely, betting quotients which do fulfil both premises do not fulfil the sum condition. Therefore, although the Dutch Book argument is logically valid *under* its own conditions, *because* these conditions are unrealistic it has no descriptive or prescriptive significance. Hence it is illusion to want to measure subjective probabilities of singular events by means of betting quotients so that they fulfil the axioms of the calculus of probability.

5 STRICTLY COHERENT BETTING QUOTIENTS IN CARNAP II

This criticism of de Finetti applies equally to Carnap's position in his last published paper, for here, stimulated by the articles of Kemeny, Lehmann and Shimony, Carnap adopts de Finetti's theorem (though with certain technical modifications). Carnap discusses[2] how, by choosing negative gross gains S_i instead of positive ones, a bet on an event or an hypothesis can be transformed into a bet against. However, he is even more removed from the practice of actual bookmaking than is de Finetti. He no longer requires that the opponent first bets and then receives gross gains later (or, in the case of negative stakes, that the individual pays in, later receiving the gross gains from the opponent), so that one would first have to calculate the net gains. Instead, the net gains change hands only *after* the betting has been decided. The individual now should be willing to change betting direction, in accordance with condition 3 of the coherence-theorem, if he considers one (!) betting quotient to be fair. Thus, the *consequences* of the fairness of betting quotients are the same in

[1] Shimony [1967], p. 331, and Baillie [1973], p. 395.

[2] See Carnap [1971*b*] pp. 106–7.

Carnap II as they are in Carnap I. But since he does not, and probably cannot indicate the ***requirements*** of fairness any longer, the mention of fair betting quotients is just a vague reminder of his earlier exposition, particularly because the willingness of the individual to exchange roles is no longer important. While de Finetti, in accordance with condition 3, allows the alert opponent to choose positive or negative stakes to his advantage, depending upon whether the sum was less than or greater than one, Carnap II eliminates the opponent completely as an active being. He does this by introducing the trick of making the signs of the gross gains dependent automatically upon the sum of the betting quotients. For this purpose he introduces the auxiliary quantity $d = \Sigma q_i - 1$ and, to prove that coherent betting quotients fulfil his basic axiom A 3 (corresponding to the sum condition) continues: 'Then either $d > 0$ or $d < 0$; in the latter case $S < 0$.'[1] The second part of the sentence can be understood only if one has learned from a diagram[2] that the gross gain is defined as $S = 1/d$, so that it is negative if $\Sigma q_i < 1$, and positive if $\Sigma q_i < 1$. This would seem to contradict de Finetti's maxims. But Carnap speaks about stakes and gains of the ***individual***, and if these are seen from the perspective of de Finetti's opponent, then it is clear that the signs are inverted. Hence, Carnap's results are identical here with those of de Finetti.

In conclusion I shall discuss a ***second negative rationality premise*** which up until now has not been considered. In his review of de Finetti in 1955 Shimony suggests a sharpening of the concept of coherence. Since that time it has been dealt with under various names, collected together by Carnap II under the terms 'coherence I' and 'coherence II'.[3] De Finetti's coherent betting quotients prohibit a situation in which all bets together could lead to a certain loss by the individual. This would happen in actual bookmaking, when the sum of the stakes is smaller than the smallest gross gain and, hence, all $G_i > 0$. Following Ellis,[4] I shall designate such a Dutch Book, the only kind that we have considered until now, as a Dutch Book$_1$ parallel to the distinction between coherence I and coherence II. If this situation is avoided, however, it does not necessarily follow that the individual can win in at least one case. Rather, even when not all G_i can be > 0 because of $\Sigma q_i \geqslant 1$, it is still possible to imagine another case whereby the individual 'can at best lose nothing, and in at least one possible eventuality he will suffer a positive loss'. Shimony who wrote this in his [1955], seems to have been the one to discover this possibility.[5] This case, which is not quite so unfavourable for the bookmaker, but does not allow a positive gain in the long run, occurs if the sum of the stakes is exactly the same as the smallest gross gain. Thus (at least) one G_i is equal to zero, while the others are positive. But of course such a Dutch Book_2 should also be avoided. And if betting quotients fulfil this condition, that is, of excluding not only Dutch Books_1, but Dutch Books_2 as well, they are, as Carnap II calls them, ***strictly coherent.*** Kemeny, alluding to Shimony's idea, spoke in 1954 of 'fair' and 'strictly fair' (probably having the fair betting quotients of Carnap I in mind) and found these terms 'more suggestive'.[1] Carnap II on the contrary finds them 'misleading'.[2]

---

[1] *Ibid.*, p. 110.

[2] *Ibid.*, p. 112.

[3] *Ibid.*, p. 114.

[4] Ellis [1973], p. 131.

[5] See *op. cit.*, p. 9.

Since for Dutch Books_2 there is less possibility for varying the gross gains than with Dutch Books_1, one would expect that coherence II imposes more conditions on the betting quotients than coherence I. But even with Shimony and Kemeny, if I am correct, strictly coherent betting quotients fulfil exactly the same standard axioms of the probability calculus as quotients which are simply coherent. To be sure, Carnap II introduces an additional axiom A 5 of ***regularity*** which is supposed to be a necessary prerequisite of coherence II: 'If (the credence function) C is not regular, it is not strictly coherent,' and 'Regularity . . . is equivalent to strict coherence.'[3] But the regularity axiom in effect only requires that each individual betting quotient be greater than zero, and this was already (as indicated above on page 334) necessary for the proof of the simple coherence-theorem, since the opponent must bet on all events. If not, Dutch books are impossible regardless of the sum of the betting quotients because the opponent would suffer a loss if an event occurs on which he had ***not*** placed a bet. Therefore, coherent as well as strictly coherent betting quotients must all be greater than zero. However, this in a certain sense contradicts the axioms of probability, in particular, Carnap's Axiom of the lower bound (which is zero), following his A 1.[4]

The prescription that Dutch Books_2, as well as Dutch Books_1 should rationally be avoided, has led to the plausible idea of trying to specify the requirements of betting quotients. This idea, however, has not been realised, and is probably not realisable since it is not possible to distinguish strictly coherent betting quotients from simple coherent ones—whether in Carnap's case, or with actual bookmakers. This is a point that will become clear from the following.

Let us return to the proof of coherence I in bookmaking. The proposition states that if $\Sigma q_i > 1$ then not all G_i can be > 0. The proof shows that all G_i and therefore $\Sigma q_i G_i$ can be > 0 only if $\Sigma q_i < 1$. The corresponding proposition for coherence II says that if $\Sigma q_i \geqslant 1$ not all G_i can be $\geqslant 0$

---

[1] Kemeny [1963], p. 719.

[2] Carnap [1971*b*], p. 115.

[3] *Ibid.*, p. 110 and p. 114.

[4] *Ibid.*, p. 38.

and at least one $G_i > 0$ *and* at least one $G_i = 0$. To prove this one again assumes the contrary—namely, that all G_i are $\geqslant 0$, and at least one $G_i > 0$ and at least one $G_i = 0$, from which it follows that $\Sigma q_i G_i$ must be > 0; the remainder of the proof is then identical with the earlier one.

The precondition for a Dutch Book_1 as well as for a Dutch Book_2 is, therefore, that $\Sigma q_i < 1$. Conversely, the precondition for coherence I and coherence II is that $\Sigma q_i \geqslant 1$. If the latter is not the case, then the opponent can not only make a Dutch Book_2, but also a Dutch Book_1 which, clearly, he would prefer. This explains why the risk of a Dutch Book_2 plays absolutely no part in the considerations of a real bookmaker. According to de Finetti's conditions only $\Sigma q_i = 1$ remains for coherence II from which, to repeat, it does not follow even from strictly coherent betting quotients that the positive rationality premise is fulfilled, or that it even could be fulfilled. Since the observance of all three conditions would be economically quite senseless, I consider it, incidentally, doubtful that Keynes (who in his 'classical' *Treatise on Probability* insisted that not all probability statements can assume a quantitative form and whose book was, nevertheless, a source of inspiration for Carnap) 'might not be unsympathetic' to the betting quotient theory, as Braithwaite assumes in his editorial foreword to the new edition of 1973.[1] But since we cannot question Keynes, both views necessarily remain idle speculation.

CONCLUSION

The coherence-theorem has fundamental significance for the subjective theory of probability which is, in turn, important for inductive logic as developed by Carnap II and for the theory of rational decision under conditions of risk or uncertainty. If my thesis that the Dutch Book argument fails is accepted, then this could constitute a strong *technical* objection to these three theories. However, one would have to be very cautious as regards the impact of this thesis, since many people support these theories for *philosophical* reasons, about which, certainly as regards inductive logic, I have nothing new or significant to say. As far as decision theory is concerned, the reader is referred to several arguments which I developed in my dissertation.[2] Thus, I will conclude with a few remarks concerning subjective probability.

The appeal of the theory of subjective probability derives largely from its basis in the concept of probability found in everyday life, a concept which includes the probability of singular events. To be sure, in everyday life it is primarily a comparative concept to be made precise by means of coherent betting quotients. Such naive ordinary language approaches always begin anew, that is, without regard to the *history* of the subject under consideration. Hence, we find that de Finetti makes very few remarks concerning precursors, while Carnap's pertinent comments have less to do with the study of the sources than of Keynes' *Treatise*. The main historical claim made by Carnap is that the explicitly frequentist approach (developed after 1840 by Cournot, Ellis and Fries, and later by Venn, as a reaction to the subjectivism of Laplace) is an 'illegitimate usurpation',[1] since the founding fathers must have meant something like probability$_1$.

---

[1] Braithwaite [1973], p. xxii.

[2] Heilig [1977].

If one traces the origins of what today is called the 'probability calculus',[2] one finds the following. On the one hand, even well before 1654–7 (which is usually taken as the starting point with the works of Pascal, Fermat and Huygens) 'probabilitas' was a well-known concept that served, for example, in ranking the credibility of historical events or theological writings. And on the other hand neither Pascal nor Huygens applied either this, or an objective, or *any* kind of concept of probability, not even a purely formal one in the sense of the mathematical equipossibility definition. As Ivo Schneider has shown,[3] it was first Leibnitz in 1678 (in his 'De incerti aestimatione'), and then James Bernoulli in 1685 (in his still unpublished 'Meditationes et Annotationes') who tried to make the newly developed calculations of expectations in games of chance fruitful for the old concept of probability. Therefore, if one wishes to be pedantic, Hacking's thesis that probability emerged around 1660[4] is not correct. On the contrary, the calculus of probability emerged without any concept of probability. Since games of chance clearly lead to a frequentist concept of probability, one might say that the 'usurpation' was in the opposite direction. But, considering the very loose terminology of the seventeenth century, it is doubtful whether this assertion makes any sense.

Bernoulli later explicitly formulated the mathematical concept of probability in his [1713], giving it a subjective interpretation because of the deterministic metaphysics, as it were, of his day. He defined probability as the measure of our knowledge or ignorance of future events, which, due to divine providence and predestination, are just as certain as those of the past or of the present, for 'if that which is in the future is not certain to occur, then it is not comprehensible why the highest Creator should be accorded the glory of omniscience and omnipotence'.[5] At the same time, he developed the programme of our 'modern' decision theory based on his subjective concept; for his 'Art of Conjecturing' was intended as an aid in making better judgments and decisions, particularly in economic questions.[1]

---

[1] Carnap [1950], p. xviii of the Preface to the second edition, 1962.

[2] Important new contributions are by Byrne [1968], Schneider [1972] and Hacking [1975].

[3] Schneider [1972], pp. 34 ff,

[4] Hacking [1975], pp. 9, 11, 18 and *passim*.

[5] Bernoulli [1713], p. 211.

Bernoulli did not complete his *opus*, but his ideas continue to have an effect today. It is probably time that a history was written of the attempts, beginning with Bernoulli's, to combine the calculus of probability with the concept of subjective probability. Since it will presumably be a history of many failures, it might then be possible using both critical *and historical* arguments, to convert some subjectivists.

Frankfurt/Main

REFERENCES

BAILLIE, P. [1973]: 'Confirmation and the Dutch Book Argument', *British Journal for the Philosophy of Science*, **24**, pp. 393–7.

BERNOULLI, JAKOB [1713]: *Ars Conjectandi.*

BRAITHWAITE, R. B. [1973]: 'Editorial Foreword', in J. M. Keynes: *A treatise on probability*, Collected Writings, vol. VIII, London: Macmillan, pp. xv–xxii.

BYRNE, E. F. [1968]: *Probability and Opinion.* The Hague: Nijhoff.

CARNAP, R. [1950]: *Logical Foundations of Probability.* London: Routledge & Kegan Paul.

CARNAP, R. [1968]: 'Inductive Logic and Inductive Intuition', in I. Lakatos (*ed.*): *The Problem of Inductive Logic.* Amsterdam: North-Holland, pp. 258–67.

CARNAP, R. [1971*a*]: 'Inductive Logic and Rational Decisions', in Carnap and Jeffrey [1971], pp. 5–31.

CARNAP, R. [1971*b*]: 'A Basic System of Inductive Logic, Part I', in Carnap and Jeffrey [1971], pp. 33–165.

CARNAP, R. and JEFFREY, R. C. (*eds.*) [1971]: *Studies in Inductive Logic and Probability, Vol. I.* Berkeley, Los Angeles and London: University of California Press.

CHENERY, J. T. [1963]: *The Law and Practice of Bookmaking, Betting, Gaming and Lotteries*, second edn, London: Sweet and Maxwell.

ELLIS, B. [1973]: 'The Logic of Subjective Probability', *British Journal for the Philosophy of Science*, **24**, pp. 125–52.

DE FINETTI, B. [1931]: 'Sul Significato Soggettivo della Probabilità', *Fundamenta Mathematicae*, **17**, pp. 298–329.

DE FINETTI, B. [1937]: 'La Prévision: ses Lois Logiques, ses Sources Subjectives', *Annales de l'Institut Henri Poincaré*, **7**, pp. 1–68. English translation: 'Foresight: Its Logical Laws, Its Subjective Sources', in Kyburg and Smokler (*eds.*) [1964], pp. 93–158.

DE FINETTI, B. [1974]: *Theory of Probability*, vol. 1 (vol. 2: 1975). London: Wiley. English translation of *Teoria delle Probabilità*, Turin, 1970.

GILLIES, D. A. [1972]: 'The Subjective Theory of Probability', *British Journal for the Philosophy of Science*, **23**, pp. 138–56. Review of de Finetti [1974].

HACKING, I. [1967]: 'Slightly More Realistic Personal Probability', *Philosophy of Science*, **34**, pp. 311–25.

HACKING, I. [1968]: 'On Falling Short on Strict Coherence', *Philosophy of Science*, **35**, pp. 284–6.

HACKING, I. [1973]: 'Propensities, Statistics and Inductive Logic', in P. Suppes *et al.* (*eds.*): *Logic, Methodology and Philosophy of Science IV.* Amsterdam: pp. 485–500.

HACKING, I. [1975]: *The Emergence of Probability.* Cambridge University Press.

HEILIG, K. [1975]: 'Personelle oder Statistische Wahrscheinlichkeit?—Einige Bemerkungen zu Stegmüller', *Allgemeines Statistisches Archiv*, **59**, pp. 375–91.

HEILIG, K. [1977]: *Einmaligkeit und Wiederholung in der Entscheidungstheorie—Eine Kritik des Bernoulli-Prinzips.* Frankfurt am Main and Zürich; Harri Deutsch,

---

[1] *Ibid.*, p. 212.

KEMENY, J. G. [1955]: 'Fair Bets and Inductive Probabilities', *Journal of Symbolic Logic*, *20*, pp. 263–73.

KEMENY, J. G. [1963]: 'Carnap's Theory of Probability and Induction', in P. A. Schilpp (*ed.*): *The Philosophy of Rudolf Carnap*, La Salle, Ill. and London: Open Court and Cambridge University Press, pp. 711–38.

KYBURG, H. E. and SMOKLER, H. E. (*eds.*) [1964]: *Studies in Subjective Probability*. New York: Wiley.

LEHMANN, R. S. [1955]: 'On Confirmation and Rational Betting', *Journal of Symbolic Logic*, **20**, pp. 251–62.

VON MISES, R. [1957]: *Probability, Statistics and Truth*. Second revised English edition, edited by Hilda Geiringer, London: Allen and Unwin.

MITCHELL, J. [1972]: *Betting*. London: Pelham.

RAMSEY, F. P. [1931]: 'Truth and Probability' in F. P. Ramsey: *The Foundations of Mathematics and Other Logical Essays*, edited by R. B. Braithwaite, London and New York: Kegan Paul, pp. 156–98. Page numbers refer to the reprint in Kyburg and Smokler (*eds.*) [1964], pp. 61–92.

SCHNEIDER, I. [1972]: *Die Entwicklung des Wahrscheinlichkeitsbegriffs in der Mathematik von Pascal bis Laplace*, Habilitationsschrift, Mathematische Fakultät, Universität München.

SHIMONY, A. [1955]: 'Coherence and the Axioms of Confirmation', *Journal of Symbolic Logic*, **20**, pp. 1–28.

SHIMONY, A. [1967]: 'Amplifying Personal Probability', *Philosophy of Science*, **34**, pp. 326–33.

SIDNEY, C. [1976]: *The Art of Legging*. London: Maxline International.

STEGMÜLLER, W. [1973]: *Personelle und Statistische Wahrscheinlichkeit*, two volumes. *Probleme und Resultate der Wissenschaftstheorie und Analytischen Philosophie*, IV. Berlin: Springer.

VICKERS, J. M. [1976]: *Belief and Probability:* Dordrecht and Boston: D. Reidel Publishing Co.

Brit. J. Phil. Sci. 29 (1978), 347–361 *Printed in Great Britain*

Discussion Notes

IS PLANCK'S 'PRINCIPLE' TRUE?

I think that Thomas Kuhn and many of his followers have been misled by Planck's much-quoted observation and have treated it as more reliable and important than it is.[1]

> A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it.[2]

In point of fact, Planck was an exception to his own 'principle', many of his most illustrious colleagues were too, and the relativistic implications which Kuhn and others have drawn from it are ***diametrically opposed*** to Planck's specific comments on that subject. In short, Planck's 'principle' was a mere ***obiter dictum*** which reflected neither his most fundamental beliefs nor the scientific practice of himself or the great men around him.

Max Planck's most outstanding contribution to science, his quantum theory of 1900, required him to accept Boltzmann's constant and the latter's statistical interpretation of the second law of thermodynamics, ***which Planck had specifically opposed for over twenty years, in fact since his own doctoral dissertation of 1879.***[3] Furthermore, in light of Kuhn's theory of 'scientific revolutions' and 'incommensurable paradigms', it is interesting to point out that Planck had the strength of character not only to admit he was wrong in opposing Boltzmann but to ***start*** the quantum tack of 'modern physics' and become the ***first major supporter*** of Einstein's special theory. He bridged the 'incommensurabilities' of 'classical' and 'modern' physics, contributed to old and new, supported the soundest features of both, and perhaps most important ***vis-à-vis*** Kuhn ***understood both.***

Nor was Planck an isolated example. His teacher Helmholtz, the co-founder of physical chemistry Ostwald, and the brilliant Werner Heisenberg all were persuaded by evidence and reason to change their minds on the very issues of deepest concern to them and intimately connected with their greatest contributions to science.

Hermann Helmholtz was a determined opponent of Young's three-colour theory of perception in 1852 but by 1858 had altered his opinion on scientific grounds until he became its chief advocate such that it is now called 'The Young–Helmholtz Theory'.[4] Not only was this patriotic Prussian able to summon the courage to admit he had been wrong, but in switching to the 'English' approach of Newtonian optics joined to Young's colour theory he had to face the powerful opposition of traditional 'German' sources such as the ideas of Goethe and Müller and later those of Hering and Mach.

Wilhelm Ostwald, the famous 'energeticist' and with Mach the ***leading*** opponent of the atomic theory in chemistry and physics since 1892, was influenced to change his mind in 1908 and accept the indispensability of the

---

[1] Kuhn [1973], p. 151.

[2] Planck [1950], pp. 33–34.

[3] Planck [1950], pp. 18, 31–33 and 41–43.

[4] Turner [1972], pp. 246–47.

'atomic hypothesis' as the result of studying the work of Einstein and Perrin on Brownian motion.[1]

Werner Heisenberg after his development of matrix mechanics in 1925 and his inexactness (not 'uncertainty') principle in 1927 was persuaded much against his will by Niels Bohr's almost ruthless argumentation to a 'compromise' in which Bohr's complementarity principle was accepted as at least as fundamental as his own in the foundations of quantum theory such that both are commonly viewed as the primary conceptual pillars on which the Copenhagen interpretation of quantum mechanics rests.[2]

In short, Planck's 'principle' was not true for Planck, Helmholtz, Ostwald, or Heisenberg in the most critical aspects of their work, and it is doubtful if it is true at all in unqualified form or even in a qualified way for men of strong intellectual integrity who insist on carefully examining each relevant theory and the pertinent experimental data.

As for the relativistic implications which Kuhn has apparently inferred from Planck, it should also be noted that, ***unlike Kuhn***, Planck *does* distinguish between Einstein's physical doctrine of relativity and the alleged relativity of truth on the one hand and between relative certainty and relative truth on the other. Like Einstein himself, Planck accepted physical relativity and the notion of relative certainty along with human fallibility but ***flatly rejected*** the notion that truth is relative or is merely a matter of 'coherence' or 'pragmatic utility'. Truth was not merely 'an internal feature of incommensurable paradigms'; it was a ***transcendent*** correspondence with a reality which exists independently of our perception and understanding of it. Both Planck and Einstein held that while our understanding of truth may change, truth itself is absolute. Indeed, the search for what is absolute is the highest and noblest scientific goal.

According to Einstein in a letter to Schlick:

> In general your presentation fails to correspond to my conceptual style insofar as I find your whole orientation so to speak too positivistic. . . . I tell you straight out: physics is the attempt at the conceptual construction of a model of the ***real world*** and of its lawful structure. You will be astonished about the 'metaphysicist' Einstein. But every four- and two-legged animal is *de facto* in this sense metaphysicist.[3]

Planck wrote in his scientific autobiography:

> In the opening paragraph of this autobiographical sketch, I emphasized that I had always looked upon the search for the absolute as the noblest and most worthwhile task of science. The reader might consider this contradictory to my avowed interest in the Theory of Relativity. But it would be fundamentally erroneous to look at it that way. For everything that is relative presupposes the existence of something that is absolute, and is meaningful only when juxtaposed to something absolute. The often heard phrase, 'Everything is relative', is both misleading and thoughtless . . . . The Theory of Relativity confers an absolute meaning on a magnitude which in classical theory has only a relative significance: the velocity of

---

[1] Sommerfeld [1944], p. 26.

[2] Jammer [1974], p. 69.

[3] Holton [1968], p. 660

light. The velocity of light is to the Theory of Relativity as the elementary quantum of action is to the Quantum Theory; it is its absolute core.[1]

JOHN T. BLACKMORE
University of Cambridge

REFERENCES

GOLDBERG, S. [1976]: 'Max Planck's Philosophy of Nature and His Elaboration of the Special Theory of Relativity', *Historical Studies in the Physical Sciences*, **7**, pp. 125–60.
HOLTON, G. [1968]: 'Mach, Einstein, and the Search for Reality', *Daedalus*, **97**, pp. 636–73.
JAMMER, M. [1974]: *The Philosophy of Quantum Mechanics.*
KUHN, T. S. [1973]: *The Structure of Scientific Revolutions.*
PLANCK, M. [1950]: *Scientific Autobiography and Other Papers.*
PLANCK, M. [1965]: *Vorträge und Erinnerungen.*
SOMMERFELD, A. [1944]: 'Ludwig Boltzmann zum Gedächtness', *Wiener Chemiker Zeitung*, **47**, pp. 25–26.
TURNER, R. S. [1972]: 'Helmholtz, Hermann von', *Dictionary of Scientific Biography*, **6**, pp. 246–7.

THE CLOCK HYPOTHESIS AND THE LORENTZ TRANSFORMATIONS

INTRODUCTION

In his [1975] review of Professor Dingle's book: *Science at the Crossroads*, Professor Whitrow wrote:

> Dingle has other reasons for rejecting special relativity. In particular he queries the statement made by Einstein in his famous paper of [1905], that "a balance-clock at the equator must go more slowly, by a very small amount, than a precisely similar clock situated at one of the poles under otherwise identical conditions". Dingle asks how is it possible from Einstein's theory to conclude that the equatorial and not the polar clock works more slowly? He complains that on this question his critics have been silent. This question is a side issue compared with the problem considered above, but it is unfortunate that no one has bothered to discuss it.

The aim of this note is to discuss the relation of the clock hypothesis to the Lorentz transformations.

The Lorentz transformations are precise statements relating the ***measurements*** of the positions and times of events in two inertial reference frames. If S and S' are two inertial reference frames, with S' moving with uniform velocity v relative to S along their common x axis, and if (x, y, z, t) and (x', y', z', t') are the coordinates and time of an event measured relative to S and S' respectively, according to the Lorentz transformations

$$x' = \gamma(x - vt); \quad y' = y; \quad z' = z$$
$$t' = \gamma(t - vx/c^2); \quad \gamma = 1/(1 - v^2/c^2)^{\frac{1}{2}}$$

---

[1] Planck [1950], pp. 46 and 47.

The same fundamental units of length and time must be used in S and S', and all the measurements of the positions and times of events must be carried out in a way consistent with special relativity, for example using stationary rulers and clocks synchronised using light signals and the principle of the constancy of the speed of light.

1 CLOCKS MOVING WITH UNIFORM VELOCITY

Consider a clock at rest at x', y' z' in S'. Consider two events, which are the two successive ticks on the clock at rest in S'. Let the times of these two ticks be at t' and $(t'+\delta t')$ in S'. Since the clock at rest in S' is just as good as any other of the synchronised clocks at rest in S' to measure time intervals in S', one is justified in using the times on the clock at rest at x', y', z' in S' to represent *both* the readings of the clock itself, and the times of the two events (ticks) measured relative to S' to be used in the Lorentz transformations. According to the Lorentz transformations, the times of these events, measured relative to S, should be $t=\gamma(t'+vx'/c^2)$ and $(t+\delta t)=\gamma(t'+\delta t'+vx'/c^2)$. Subtracting these equations gives $\delta t=\gamma\delta t'$. Hence, according to the Lorentz transformations, δt the time between the two ticks on the clock at rest in S', measured by spatially separated synchronised clocks at rest in S, is equal to $\gamma(\delta t')$. For example if $\delta t'$ is one second, δt is γ seconds. This is the standard formula for time dilation. By considering a clock at rest in S, it can be shown that time dilation is reciprocal. Thus the Lorentz transformations can be used to relate the rates of clocks which are moving with *uniform* velocity or are at rest relative to inertial reference frames.

2 ACCELERATING CLOCKS

Consider a clock which is accelerating and moving relative to both S and S'. Let the instantaneous values of the speed of this clock, measured relative to S and S', be u and u' respectively. Let the accelerating clock be instantaneously at rest in the inertial reference frame S°. If the first tick on the accelerating clock is recorded at x, y, z at a time t relative to S, the position and time of this event relative to S' and S° can be predicted using the Lorentz transformations. Since the rate of an accelerating clock may be affected by its acceleration, one cannot, without making an extra assumption, relate the time between the two ticks on the accelerating clock, measured by the readings of the accelerating clock, to the time interval between these same two events, measured by an observer at rest in S° using a clock at rest in S° and which coincides with the accelerating clock. (The Lorentz transformations only relate the coordinates and time of the first and second events (ticks) measured by rulers and synchronised clocks at rest in S, S' and S°.) The clocks hypothesis is introduced to relate the readings of the accelerating clock to the readings of the clocks at rest in S, S' and S°. According to the clock hypothesis, it is postulated that the rate of the accelerating clock is independent of its acceleration and equal to the rate of an identical clock, which is permanently at rest in the inertial reference frame S° in which the accelerating clock is instantaneously at rest. If $\delta\tau$ is the time between the two ticks on the accelerating clock, measured by the accelerating clock itself, according to the clock hypothesis the time δt° between these two ticks measured by a clock, permanently at rest in the inertial reference frame S° and which coincides with the accelerating clock, is also equal to $\delta\tau$. The Lorentz transformations can be used to relate the time interval δt° between the two ticks, measured by the clock permanently at rest in the inertial reference frame S° to the time interval between the same two events, measured by spatially separated synchronised clocks at rest relative to any other inertial frame such as S or S'. Algebraic application of the Lorentz transformations (see my [1964] for details) shows that the interval δs between these two events, is an invariant in all inertial reference frames, so that

$$\begin{aligned}\delta s^2 = -c^2(\delta t^\circ)^2 &= \delta x^2+\delta y^2+\delta z^2-c^2\delta t^2\\ &= \delta x'^2+\delta y'^2+\delta z'^2-c^2\delta t'^2\end{aligned}$$

According to the clock hypothesis $\delta t^\circ = \delta\tau$. Hence relative to S we should have

$$-c^2(\delta\tau)^2 = \delta t^2\left[\left(\frac{\delta x}{\delta t}\right)^2+\left(\frac{\delta y}{\delta t}\right)^2+\left(\frac{\delta z}{\delta t}\right)^2-1\right]$$

$$\delta\tau = \delta t(1-u^2/c^2)^{\frac{1}{2}} \tag{1}$$

Similarly relative to S', according to the clock hypothesis,

$$\delta\tau = \delta t'(1-u'^2/c^2)^{\frac{1}{2}} \tag{2}$$

Integrating equations (1) and (2) along the path of the accelerating clock gives

$$\int d\tau = \int dt(1-u^2/c^2)^{\frac{1}{2}} \tag{3}$$

$$\int d\tau = \int dt'(1-u'^2/c^2)^{\frac{1}{2}} \tag{4}$$

Equations (3) and (4) are the mathematical representations of the clock hypothesis. According to the clock hypothesis the time of the journey measured by the accelerating clock itself is given by $\int d\tau$, whereas the time of the same journey measured by spatially separated synchronised clocks at rest in S and S' are $\int dt$ and $\int dt'$ respectively. Since the interval δs is an invariant, equation (1) transforms into equation (2) under a Lorentz transformation, so that equations (1) and (2) and hence equations (3) and (4) are *Lorentz covariant*. Since equations (3) and (4) are Lorentz covariant, it is consistent with the theory of special relativity to postulate the clock hypothesis. Whether or not the clock hypothesis holds in practice must ultimately be decided by experiment.

3 EXPERIMENTAL CHECK OF THE CLOCK HYPOTHESIS

If a box of radioactive particles is at *rest* in the laboratory system S, according to the law of radioactive decay the number left after time t measured in the laboratory system, is given by

$$N = N_0 \exp(-t/T) \tag{5}$$

where N_0 is the number present at time $t = 0$ and T is the mean lifetime of the radioactive particles measured in the inertial reference frame in which they are at rest. By measuring the number of radioactive particles left, the time t can be determined. Thus a box of radioactive particles can be used as a radioactive clock to measure time intervals.

If accelerating radioactive clocks are to be consistent with the clock hypothesis, the rate of decay of radioactive particles inside an accelerating box, measured in the inertial frame S° in which the particles are instantaneously at rest, should be independent of the accelerations of the particles. The number decaying in a time $\mathrm{d}t^\circ$, measured in the inertial reference frame S° in which the accelerating radioactive particles are instantaneously at rest, should therefore be given by

$$\mathrm{d}N = -\frac{N}{T}\,\mathrm{d}t^\circ = \frac{-\mathrm{d}t(1-u^2/c^2)^{\frac{1}{2}}N}{T} \tag{6}$$

If the box of radioactive particles is moving with uniform speed $u = v$ in a circle, integrating equation (6) gives

$$N = N_0 \exp\left(-t/\gamma T\right) \tag{7}$$

The experimental value for the mean lifetime of μ-mesons *at rest* in the laboratory is $T = 2.20 \times 10^{-6}\ s$. In their experiment, Farley, Bailey and Picasso (see their [1968]) sent μ-mesons of momenta 1·274 GeV/c, corresponding to $\gamma = 12{\cdot}14$ around in circles of $5m$ diameter in a magnetic field. They found ***experimentally*** that for these μ-mesons, if t is in seconds,

$$N = N_0 \exp\left[-t/(26{\cdot}15 \pm 0{\cdot}03) \times 10^{-6}\right] \tag{8}$$

According to equation (7), the expected value for the mean lifetime of the moving μ-mesons measured relative to the laboratory system should be $\gamma T = 26{\cdot}72 \times 10^{-6}\ s$. The difference of 2 per cent was interpreted in terms of losses due to imperfections in the magnetic field. Comparing equations (5) and (8), it can be seen that, if one had a box containing N_0 μ-mesons at rest in the laboratory, and, if one sent a box also containing N_0 μ-mesons on a journey at high speed in a circular path relative to the laboratory, there would be more μ-mesons left, on average, in the box moving relative to the laboratory when the boxes coincided again, the difference being consistent with the clock hypothesis, equation (3). There is therefore a ***physical difference*** associated with the accelerated motion of a radioactive clock relative to the laboratory system. This physical difference can be predicted using the clock hypothesis, equation (3). In the experiment of Farley *et al.* the centripetal accelerations of the μ-mesons was $\sim 10^{15}$ g. (A balance clock would of course not stand up to such accelerations.) This experiment in atomic physics has shown that the clock hypothesis, equation (3), predicts the correct lifetimes for radioactive particles ***accelerating*** relative to the laboratory system.

4 DISCUSSION

For a clock at rest in S', $u' = 0$, $u = v$, $\int \mathrm{d}\tau = \int \mathrm{d}t'$, and equation (3) gives $\delta t = \gamma \delta t'$. This is the normal formula for time dilation derived previously using the Lorentz transformations. For the case of a clock at rest in S, for which $u = 0$ and $u' = -v$, equation (4) gives the normal formula for time dilation. Thus the clock hypothesis reduces to the normal formula for time dilation for clocks at rest in either S or S'. In these latter cases, the predictions of the clock hypothesis and the Lorentz transformations are identical, and time dilation is reciprocal. Asymmetry comes in when a clock is accelerating relative to an inertial reference frame.

Summarising, the clock hypothesis goes beyond the Lorentz transformations. The clock hypothesis in the form of equation (3) is Lorentz covariant. It is in agreement with experiments on accelerating radioactive particles. In the *special* case when u is a constant, equation (3) reduces to the normal formula for time dilation. It is only the latter case which can be treated using the Lorentz transformations without making a further assumption about the behaviour of an accelerating clock. The clock hypothesis can now be looked upon as a law of nature, similar to the law of variation of inertial mass with velocity. Both laws can be developed using the theory of special relativity as a heuristic aid. Both laws have been confirmed experimentally.

W. G. V. ROSSER
Exeter University

REFERENCES

Einstein, A. [1955]: 'Zur Elektrodynamik der bewegter Körper' *Annalen der Physik*, 17, p. 891.

Farley, F. J. M., Bailey, J. and Picasso, F. [1968]: 'Experimental Verifications of the Theory of Relativity', *Nature*, 217, p. 17.

Rosser, W. G. V. [1964]: *An Introduction to the Theory of Relativity*. London: Butterworths.

Whitrow, G. J. [1975]: Review of H. Dingle: *Science at the Crossroads*, *British Journal for the Philosophy of Science*, 26, p. 358.

A DILEMMA DEFENDED

D'Agostino [1978] claims to dissolve a type of methodological problem which I present in my [1975*b*] as characteristic of linguistics (although unlikely to be restricted to that science). Linguists aim to find a general linguistic theory which will describe the class of languages usable by humans; at the same time, for each particular observed language they seek a theory which describes that language, as a class of sentences utterable by its speakers. My problem arises in cases where we have plausible general linguistic theories T, T' and plausible theories t, t' of some observed language L such that T predicts that L-as-described-by-t' is not a possible human language, T' fails to make this prediction, and t' is methodologically superior to t while T is methodologically superior to T'. It seems that we must choose between the theory-pairs $\langle T, t\rangle$ and $\langle T', t'\rangle$, but it is not clear on what grounds such a choice can be made, given that T, T', t, t' are all unrefuted.

(In his exposition of my problem d'Agostino adds the postulate that T' predicts L-as-described-by-t to be impossible. In my own example T' allowed both t and t' as descriptions of possible languages; and indeed it was the relative weakness of T', of which this was an instance, that justified the statement that T' was methodologically inferior to T. Another apparent misunderstanding is that d'Agostino represents a theory t of a language L as predicting that every sentence in the class defined by t is in L, whereas in fact t is empirical because it predicts that every sentence in L is in the class defined by t. We observe only positive instances of the membership of L, so we cannot refute t by observing that some sentence in the class defined by t fails to occur in L, but we certainly can refute t by observing that some sentence in L fails to occur in the class defined by t. *Cf.* my [1975*a*], ch. 4.)

According to d'Agostino, sound methodology directs the linguist to reject both $\langle T, t\rangle$ and $\langle T', t'\rangle$ and instead to 'try to construct' new theories T^*, t^* which are compatible with one another and are methodologically as good as T, t' respectively.

This seems to me to solve nothing, since the sub-optimality of the theory-pairs $\langle T, t\rangle$ and $\langle T', t'\rangle$ gives no guarantee that there is a new theory-pair $\langle T^*, t^*\rangle$ waiting to be constructed. If someone manages to construct $\langle T^*, t^*\rangle$, then it is uncontroversial that this theory-pair must be preferred to the alternatives. My problem arises only if, as is quite possible, no-one has succeeded in inventing theories with the properties required of $\langle T^*, t^*\rangle$. So long as no-one *has* achieved this, for all anyone knows no-one ever *will* achieve it. In that situation d'Agostino's prescription bids us reject all available theories both in the domain of L-study and in the domain of general linguistics, *i.e.* to hold no beliefs about these subjects. That strikes me as unreasonable.

Consider the implications of d'Agostino's prescription for the (not unrealistic) example I constructed in my [1975*b*]. In this example L was the language of Atlantis, supposed to have come down to us in the form of a large corpus of inscriptions which (while otherwise seeming to give a good sample of Atlantic grammar) contain no subordinate clauses. The theory t' was an accurate and elegant description of Atlantic as revealed by those data, while t resembled t' except that t failed to forbid subordination—the point being that an attractive general linguistic theory T declares that subordination occurs in all human languages. Thus t' is stronger than t because Atlantic sentences containing subordinate clauses are potential falsifiers for t' but not for t; on the other hand T is stronger than an alternative general theory T' which omits the prediction that all languages permit subordination. Now d'Agostino's prescription requires us *inter alia* to reject both t and t' and to hold that we know nothing about Atlantic grammar. In practice this recommendation would (rightly, I believe) be dismissed by linguists as bizarre, since there might be many aspects of Atlantic grammar *other than the issue of subordination* for which the data gave quite full evidence tending towards conclusions that did not conflict with any general theories. Thus we might find thousands of cases of plural nouns, all formed from the singular by adding an invariant suffix; why should we have to say that we do not know how Atlantic forms plural nouns, simply because there is a question about whether Atlantic permits subordinate clauses? Notice that it is not open to d'Agostino to say that we should hold the 'obvious' theory about the uncontroversial aspects of Atlantic (such as noun-pluralisation) and suspend belief purely with respect to the issue of subordination; to do this is to hold the theory t, which d'Agostino requires us to reject. A theory of a language is tested by trying to observe utterances which it predicts to be ungrammatical, so, by failing to forbid subordination, t leaves it open whether or not subordination occurred in Atlantic.

I believe that d'Agostino confuses two kinds of scepticism, one healthy and the other not. The healthy, Popperian kind of scepticism tells us to realise that each of our beliefs (or, at least, each of our scientific theories) may be false. The unhealthy kind of scepticism tells us not to hold any beliefs if they may be false. This latter principle is 'unhealthy', because human beings need to hold fallible beliefs in order to function successfully. (In my [1975*b*], p. 311, I gave an example of a reason why someone might 'need to know' what the correct theory of Atlantic was; if necessary, one could construct another hypothetical situation with the same methodologically-problematic features but in which the motive for finding the true theory at one or both levels was much more tangible and pressing.) What people need from the philosophy of science is a set of principles showing how to choose beliefs which are relatively 'good' in some sense; scepticism of the Popperian kind is part of a methodology which, arguably, serves just this purpose, but the stronger scepticism entailed by d'Agostino's remarks conflicts with that need.

The principle that theories should be rejected whenever they are less than ideal from the methodological point of view, regardless of whether methodologically-preferable alternatives are available, is likely in practice to require the rejection of all theories ever constructed in any domain (Lakatos [1970]). D'Agostino perhaps means only to argue that rejection of all extant alternatives is appropriate just when the methodologically-superior alternative at one level is incompatible with the methodologically-superior alternative at another level, as I suggest may be characteristic for linguistics in particular. But if agnosticism is not in general justified by the fact that all extant theories in a given domain are in some way or other methodologically imperfect, what is it about the special kind of methodological difficulties in my example that makes agnosticism permissible here?

GEOFFREY SAMPSON
University of Lancaster

REFERENCES

D'AGOSTINO, F. B. [1978]: 'Sampson's "dilemma" ', *British Journal for the Philosophy of Science*, **29**, pp. 183–4.

LAKATOS, I. [1970]: 'Falsification and the methodology of scientific research programmes' in I. Lakatos and A. Musgrave (*eds.*) *Criticism and the Growth of Knowledge*. Cambridge: University Press.

SAMPSON, G. R. [1975*a*]: *The Form of Language*. London: Weidenfeld & Nicolson.

SAMPSON, G. R. [1975*b*]: 'Theory choice in a two-level science', *British Journal for the Philosophy of Science*, **26**, pp. 303–18.

AD HOCNESS AND THE APPRAISAL OF THEORIES *

1 THE BAYESIAN ANALYSIS OF AD HOCNESS.

In a recent note Schaffner ([1974]) has given a formal discussion of the notion of *ad hoc*ness in terms of a Bayesian model for the appraisal of theories. Schaffner develops his general ideas in the context of a critique of Zahar's [1973] which was concerned with the particular problem of comparing the Einstein and

---

* I am grateful to Donald Gillies, Jon Dorling and Noretta Koertge for helpful comments and suggestions. The present work formed part of a paper read in Professor Watkins' seminar at The London School of Economics in January 1976.

Lorentz research programmes. Zahar suggests the following analysis of *ad hoc*ness[1]:

> *Ad hoc*$_1$: A theory is said to be *ad hoc*$_1$ if it has no novel consequences as compared with its predecessor.
>
> *Ad hoc*$_2$: [A theory] . . . is *ad hoc*$_2$ if none of its novel predictions have been actually 'verified'.
>
> *Ad hoc*$_3$: [A] . . . theory is said to be *ad hoc*$_3$ if it is obtained from its predecessor through a modification of the auxiliary hypotheses which does not accord with the spirit of the heuristic of the programme.

Zahar explains the meaning of novelty as follows[2]

> A fact will be considered novel with respect to a given hypothesis if it did not belong to the problem-situation which governed the construction of the hypothesis.

Schaffner begins his elucidation by discussing the notions of *ad hoc*$_1$ and *ad hoc*$_3$. The first he describes as a logical dream, since the novel consequences of a theory cannot in practice be 'surveyed', so the question of whether a theory is *ad hoc*$_1$ can only be discussed relative to the extent to which novel consequences have been looked for at the particular epoch of the evaluation. *Ad hoc*$_3$ Schaffner claims to be 'vague to the point of inapplicability'. For Schaffner *ad hoc*$_2$ is 'close to the sense in which *ad hoc* is used in science' and he announces that his Bayesian analysis will be brought to bear on this sense of *ad hoc*.

Denoting by $p(T/b\ \&\ e)$ the probability of a theory T to be true in the light of background knowledge b and the positive outcome e of some experiment not part of b, by $p(T/b)$ the prior assessment of T, by $p(e/T\ \&\ b)$ the probability of obtaining e given T and b, and by $p(e/b)$ the probability of obtaining e on the basis of background knowledge alone, Schaffner writes

$$p(T/b\ \&\ e) = \frac{p(T/b)\cdot p(e/T\ \&\ b)}{p(e/b)} \qquad (1)$$

If T explains e we can set $p(e/T\ \&\ b) = 1$ so in this case we obtain

$$p(T/b\ \&\ e) = p(T/b)/p(e/b) \qquad (2)$$

Schaffner proceeds to discuss *ad hoc*ness as a property of an hypothesis as a constituent part of a theory, but in order to keep the argument as simple as possible we shall follow Zahar in considering the *ad hoc*ness of theories. Translated into these terms Schaffner's idea is that a theory T gives an *ad hoc* explanation of an experimental result e if $p(T/b)$ is close to zero and $p(e/b)$ is significantly larger than $p(T/b)$. The argument for $p(T/b)$ being small is that it has no 'theoretical support' (or indeed empirical support other than e itself). This looks suspiciously like a reference to *ad hoc*$_3$, and Schaffner now appears to be claiming that a theory is *ad hoc*$_2$ partly in virtue of its being *ad hoc*$_3$. So he is not really giving an independent analysis of *ad hoc*$_2$ at all.

---

[1] Zahar [1973], p. 101. In his [1974] Zahar rephrases his definition in terms of a notion of empirical non-*ad hoc*ness expressed as a three-place relation between an observation statement, a theory and a heuristic. See also in this connection the very clear account of empirical support given by Worrall in his [1978]. However this more recent work has somewhat obscured the important distinctions drawn by Zahar in his [1973].

[2] Zahar [1973], p. 103.

The British Journal for the Philosophy of Science

Volume 29

Published for the
British Society for the Philosophy of Science
by Aberdeen University Press
1978

CONTENTS

ARTICLES

page

1 ON THE NEYMAN–PEARSON THEORY OF TESTING
Spencer Graves

25 THE INTERPRETATION AND JUSTIFICATION OF THE SUBJECTIVE BAYESIAN APPROACH TO STATISTICAL INFERENCE
Richard Buxton

39 ON THE IMPOSSIBILITY OF AN INFINITE PAST
G. J. Whitrow

117 IS ANY OF POPPER'S ARGUMENTS AGAINST HISTORICISM VALID?
Peter Urbach

131 A DEFENCE OF EVOLUTIONARY LAWS
A. Olding

145 COPENHAGENISM AND POPPERISM
Yehuda Freundlich

213 THREE MYTHS OF COMPUTER SCIENCE
James Moor

223 TESTABILITY OF THE ROLE OF NATURAL SELECTION WITHIN THEORIES OF POPULATION GENETICS AND EVOLUTION
Gerhard Wassermann

311 DID EINSTEIN NEED GENERAL RELATIVITY TO SOLVE THE PROBLEM OF ABSOLUTE SPACE? OR HAD THE PROBLEM ALREADY BEEN SOLVED BY SPECIAL RELATIVITY?
Jon Dorling

325 CARNAP AND DE FINETTI ON BETS AND THE PROBABILITY OF SINGULAR EVENTS: THE DUTCH BOOK ARGUMENT RECONSIDERED
Klaus Heilig

DISCUSSIONS

47 ON THE POSSIBILITY OF AN INFINITE PAST: A REPLY TO WHITROW
Karl Popper

49 EINSTEIN'S DEBT TO LORENTZ: A REPLY TO FEYERABEND AND MILLER
Elie Zahar

page

179 ACCURACY OF PREDICTION: A NOTE ON DAVID MILLER'S PROBLEM
Deryck Horton

183 SAMPSON'S 'DILEMMA'
Fred D'Agostino

243 REPLY TO L. PEARCE WILLIAMS
William Berkson

248 WILLIAMS DODGES AGASSI'S CRITICISM
Joseph Agassi

252 A REPLY TO AGASSI AND BERKSON
L. Pearce Williams

252 A REPLY TO 'SOME NEW ASPECTS OF RELATIVITY: COMMENTS ON ZAHAR'S PAPER'
Arthur Miller

256 GROUP SELECTION AND METHODOLOGICAL INDIVIDUALISM: A CRITICISM OF WATKINS
Edward Reed

262 A PECULIAR CONSEQUENCE OF NICOD'S CRITERION
Paul Horwich

347 IS PLANCK'S 'PRINCIPLE' TRUE?
John T. Blackmore

349 THE CLOCK HYPOTHESIS AND THE LORENTZ TRANSFORMATIONS
W. G. V. Rosser

353 A DILEMMA DEFENDED
Geoffrey Sampson

355 AD HOCNESS AND THE APPRAISAL OF THEORIES
Michael Redhead

REVIEW ARTICLES

61 UNRAVELLING THE WORLD KNOT: SCIENTISTS AND PHILOSOPHERS ON THE MIND–BRAIN CONTROVERSY (review of G. G. Globus, G. Maxwell and I. Savodnik (*eds.*): *Consciousness and the Brain: A Scientific and Philosophical Inquiry*)
Ronald Puccetti

68 THE DEVELOPMENT OF MATHEMATICS (review of M. Kline: *Mathematical Thought from Ancient to Modern Times*)
Donald Gillies

page

87 SCIENTIFIC REALISM AND PERCEPTION (review of J. W. Cornman: *Perception, Common Sense, and Science*)
Raimo Tuomela

185 THE WHEEL AND BEYOND (review of N. Rescher: *Methodological Pragmatism*)
Susan Haack

265 IS MIND AUTONOMOUS? (review of K. R. Popper and J. C. Eccles: *The Self and Its Brain*)
John Beloff

274 THE PREHISTORY OF CHANCE (review of I. Hacking: *The Emergence of Probability*)
Colin Howson

280 REDUCTION IN PHYSICS (review of R. M. Yoshida: *Reduction in the Physical Sciences*)
Wladylaw Krajewski

363 FAILURES IN CRITICISM: POPPER AND HIS COMMENTATORS (review of P. A. Schilpp (*ed.*): *The Philosophy of Karl Popper*)
John Mackie

375 ON A NEW THEORY OF EPISTEMIC PROBABILITY (review of Glen Shafer: *A Mathematical Theory of Evidence*)
Peter Williams

REVIEWS

GELLNER, E: *The Legitimation of Belief* review by Alasdair Macintyre, 105
AYALA, F. J. AND DOBZHANSKY, T. (*eds.*): *Studies in the Philosophy of Biology, Reduction and Related Problems* review by Edward Manier, 110
FISK, M: *Nature and Necessity* review by John Post, 189
GRENE, M: *The Understanding of Nature* review by David Hirschmann, 195
THOMASON, R. H. (*ed.*): *Formal Philosophy, Selected Papers of Richard Montague* review by Ryszard Wójcicki, 197
ACKERMANN, R. J.: *The Philosophy of Karl Popper* and JOHANSSON, I.: *A Critique of Karl Popper's Methodology* review by James Brown, 201
RESCHER, N.: *Plausible Reasoning* review by Noretta Koertge, 204
BELL, J. L. and MACHOVER, M.: *A Course in Mathematical Logic* review by Wilfred Hodges, 207
KRAJEWSKI, W.: *Correspondence Principle and Growth of Science* review by Ron Yoshida, 285
DAVIES, P. C. W.: *Space and Time in the Modern Universe* review by William Newton-Smith, 289
NARLIKAR, J.: *The Structure of the Universe* review by D. W. Sciama, 294
LUCAS, J. R.: *A Treatise on Time and Space* review by Graham Nerlich, 295

page

NERLICH, G.: *The Shape of Space* review by J. R. Lucas, 299
ROSENKRANTZ, R. D.: *Inference, Method and Decision* review by R. G. Swinburne, 301
MANNINEN, J. and TUOMELA, R. (*eds.*): *Essays on Explanation and Understanding* review by David Papineau, 304
CHALMERS, A. F.: *What is this Thing Called Science?* review by Gunner Andersson, 389
PORTER, R.: *The Making of Geology* review by A. Hallam, 392
BODEN, M.: *Artificial Intelligence and Natural Man* review by D. M. Mackay, 394
WESTMAN, R. S. (*ed.*): *The Copernican Achievement* review by J. R. Ravetz, 395
STROUD, B.: *Hume* review by Leslie Stevenson, 398
ENGLHARDT, H. T. and SPICKER, S. F. (*eds.*): *Evaluation and Explanation in the Biomedical Sciences* and *Philosophical Dimensions of the Neuro-Medical Sciences* review by Caroline Whitbeck, 399

MISCELLANEOUS

116 AUSTRALASIAN ASSOCIATION FOR LOGIC
116 BRITISH SOCIETY FOR THE PHILOSOPHY OF SCIENCE, PROGRAMME OF MEETINGS 1977–78

209 BRITISH SOCIETY FOR THE PHILOSOPHY OF SCIENCE, ANNUAL CONFERENCE

209 AUSTRIAN WITTGENSTEIN SOCIETY

210 BOSTON UNIVERSITY: BOSTON COLLOQUIUM FOR THE PHILOSOPHY OF SCIENCE, 1977–78

407 RECENT PUBLICATIONS

INDEX OF AUTHORS

ARTICLES

Buxton, R. The Interpretation and Justification of the Subjective Bayesian Approach to Statistical Inference, 25
Dorling, J. Did Einstein Need General Relativity to Solve the Problem of Absolute Space? Or had the Problem Already Been Solved by Special Relativity?, 311
Freundlich, Y. Copenhagenism and Popperism, 145
Graves, S. On the Neyman–Pearson Theory of Testing, 1
Heilig, K. Carnap and de Finetti on Bets and the Probability of Singular Events: The Dutch Book Argument Reconsidered, 325
Moor, J. Three Myths of Computer Science, 213
Olding, A. A Defence of Evolutionary Laws, 131
Urbach, P. Is Any of Popper's Arguments Against Historicism Valid? 117
Wassermann, G. Testability of the Role of Natural Selection Within Theories of Population Genetics and Evolution, 223
Whitrow, G. J. On the Impossibility of an Infinite Past, 39

DISCUSSIONS

Agassi, J. Williams Dodges Agassi's Criticism, 248
D'Agostino, F. Sampson's 'Dilemma', 183
Berkson, W. Reply to L. Pearce Williams, 243
Blackmore, J. T. Is Planck's 'Principle' True?, 347
Horton, D. Accuracy of Prediction: A Note on David Miller's Problem, 179
Horwich, P. A Peculiar Consequence of Nicod's Criterion, 262
Miller, A. A Reply to 'Some New Aspects of Relativity: Comments on Zahar's Paper', 252
Popper, K. R. On the Possibility of an Infinite Past: A Reply to Whitrow, 47
Redhead, M. Ad Hocness and the Appraisal of Theories, 355
Reed, E. Group Selection and Methodological Individualism: A Criticism of Watkins, 256
Rosser, W. G. V. The Clock Hypothesis and the Lorentz Transformations, 349
Sampson, G. A Dilemma Defended, 353
Williams, L. P. A Reply to Agassi and Berkson, 252
Zahar, E. Einstein's Debt to Lorentz: A Reply to Feyerabend and Miller, 49

REVIEW ARTICLES

Beloff, J. Is Mind Autonomous? (review of K. R. Popper and J. C. Eccles: *The Self and Its Brain*), 265
Gillies, D. The Development of Mathematics (review of M. Kline: *Mathematical Thought from Ancient to Modern Times*), 68
Haack, S. The Wheel and Beyond (review of N. Rescher: *Methodological Pragmatism*), 185

Howson, C. The Prehistory of Chance (review of I. Hacking: *The Emergence of Probability*), 274
Krajewski, W. Reduction in Physics (review of R. M. Yoshida: *Reduction in the Physical Sciences*), 280
Mackie, J. Failures in Criticism: Popper and His Commentators (review of P. A. Schilpp (*ed.*): *The Philosophy of Karl Popper*), 363
Puccetti, R. Unravelling the World Knot: Scientists and Philosophers on the Mind–Brain Controversy (review of G. G. Globus, G. Maxwell and I. Savodnik (*eds.*): *Consciousness and the Brain: A Scientific and Philosophical Inquiry*), 61
Tuomela, R. Scientific Realism and Perception (review of J. W. Cornman: *Perception, Common Sense and Science*), 87
Williams, P. On a New Theory of Epistemic Probability (review of Glen Shafer: *A Mathematical Theory of Evidence*), 375

REVIEWS

Andersson, G.	A. F. Chalmers: *What is This Thing Called Science?*, 389
Brown, J.	R. J. Ackermann: *The Philosophy of Karl Popper* and I. Johansson: *A Critique of Karl Popper's Methodology*, 201
Hallam, A.	R. Porter: *The Making of Geology*, 392
Hirschmann, D.	M. Grene: *The Understanding of Nature*, 195
Hodges, W.	J. L. Bell and M. Machover: *A Course in Mathematical Logic*, 207
Koertge, N.	N. Rescher: *Plausible Reasoning*, 204
Lucas, J. R.	G. Nerlich: *The Shape of Space*, 299
MacIntyre, A.	E. Gellner: *The Legitimation of Belief*, 105
MacKay, D. M.	M. Boden: *Artificial Intelligence and Natural Man*, 394
Manier, E.	F. J. Ayala and T. Dobzhansky (*eds.*): *Studies in the Philosophy of Biology, Reduction and Related Problems*, 110.
Nerlich, G.	J. R. Lucas: *A Treatise on Time and Space*, 295
Papineau, D.	J. Manninen and R. Tuomela (*eds.*): *Essays on Explanation and Understanding*, 304
Post, J.	M. Fisk: *Nature and Necessity*, 189
Ravetz, J. R.	R. S. Westman (*ed.*): *The Copernican Achievement*, 395
Sciama, D. W.	J. Narlikar: *The Structure of the Universe*, 294
Newton-Smith, W.	P. C. W. Davies: *Space and Time in the Modern Universe*, 289
Stevenson, L.	B. Stroud: *Hume*, 398
Swinburne, R. G.	R. D. Rosenkrantz: *Inference, Method and Decision*, 301
Whitbeck, C.	H. T. Engelhardt and S. F. Spicker (*eds.*): *Evaluation and Explanation in the Biomedical Sciences* and *Philosophical Dimensions of the Neuro-Medical Sciences*, 399
Wójcicki, R.	R. H. Thomason (*ed.*): *Formal Philosophy, Selected Papers of Richard Montague*, 197.
Yoshida, R.	W. Krajewski: *Correspondence Principle and Growth of Science*, 285

The lack of novelty in the prediction of e is associated by Schaffner with a high value for $p(e/b)$. On Zahar's account this is a necessary condition for lack of novelty, but not a sufficient condition. We proceed to show how using Zahar's notion of novelty one can get an 'internal' Bayesian analysis of *ad hoc*$_3$.

We express Bayes's theorem in the following familiar way

$$p(T/b\ \&\ e) = \frac{p(T/b)\cdot p(e/T\ \&\ b)}{p(e/T \& b)\cdot p(T/b) + p(e/{\sim}T \& b).p({\sim}T/b)} \tag{3}$$

where ${\sim}T$ denotes the negation of T.

We follow Schaffner in interpreting $p(A/B)$ as the degree of belief that A is true given that B is true. Note also that e is supposed in equation (3) to refer to a prediction made by the theory T (we could perhaps write e_T to emphasise this important point) so $p(e/{\sim}T\ \&\ b)$ means the probability that the prediction e_T derived from the theory T is a true prediction given that the background information b is true but the theory T is false (*i.e.* its consequences are not guaranteed to be true although 'by accident' they may be true).

Writing $p(T/b) = x$, $p(e/{\sim}T\ \&\ b) = \epsilon$
and taking $p(e/T\ \&\ b) = 1$ and using $p({\sim}T/b) = 1-x$

$$p(T/b\ \&\ e) = \frac{x}{x+\epsilon(1-x)} \tag{4}$$

We define an enhancement ratio γ by

$$\gamma = \frac{p(T/b\ \&\ e)}{p(T/b)}$$

whence using (4) we obtain the simple result

$$\gamma = \frac{1}{x+\epsilon(1-x)} \tag{5}$$

We can now explain that if a theory T is *ad hoc*$_3$ with respect to the experiment e then $\epsilon = 1$, *i.e.* the explanation of e by T in no way depends on the truth or falsehood of T, both of which eventualities lead with certainty to the result e. This is just what a scientist means when he says T was an *ad hoc* explanation of e, namely T was devised for the express purpose of explaining e, so the explanation of e is guaranteed independently of whether T is true or false. To show the consistency of our analysis if we put $\epsilon = 1$ in (5) we get $\gamma = 1$, so the posterior and prior probabilities of T are equal (there is no enhancement) and this again is just what we expect from an *ad hoc* explanation of e, namely e itself gives us no additional information for assessing the truth of T.[1]

Notice that $p(e/b) = x+\epsilon(1-x)$ is equal to unity if $\epsilon = 1$, but that $p(e/b) = 1$ is achieved for $x = 1$ whatever the value of ϵ, so the explication of novelty in terms of low $p(e/b)$ (Schaffner) and small ϵ (Zahar) are by no means equivalent.

On our analysis if $x \ll \epsilon \ll 1$, then $\gamma \simeq 1/\epsilon$, so in this case we get a big enhancement and the theory is far from being *ad hoc*. Noting that under these conditions $p(e/b) \simeq \epsilon$, we see that this is a situation in which Schaffner would claim that the theory was *ad hoc*, which highlights the way in which his analysis differs from ours. Effectively Schaffner requires γx to be small as his condition of *ad hoc*ness. He is thus concerned with the absolute value of the probability of a theory after it has explained some experimental finding. If this absolute value is still small the theory is to be regarded as an *ad hoc* explanation of the experiment. On our account the important aspect in assessing *ad hoc*ness for this case is not the absolute value of the probability but the enhancement ratio. Our point against Schaffner is not that his analysis may not explicate some legitimate sense of the ambiguous appellation *ad hoc*, but that it fails to explicate Zahar's very important notion of *ad hoc*$_2$. There is no inconsistency here on Schaffner's part, since he finds Zahar's account of *ad hoc*$_2$ inadequate in respect of the definition of novelty involved with its historical associations, but we would maintain that Zahar's sense of *ad hoc*$_2$ is the one that ought to be explicated since it is the one most importantly used in science.

---

[1] What we have shown is that $\epsilon = 1$ is a necessary condition for T to be an *ad hoc*$_3$ explanation of e. To justify $\epsilon = 1$ as a sufficient condition we must invoke a principle of insufficient reason, *viz.*, if there is no reason for the community of scientists to entertain with non-vanishing prior probability only theories which explain e, then they will not so constrain their choice of alternative theories.

2 THE CASE OF MULTIPLE PREDICTIONS

To develop our analysis a little further we can consider how a theory builds up a favourable appraisal as it makes a number of successful predictions $e_1, e_2 \dots e_n$ say. Denote by p_s the posterior probability $p(T/b \,\&\, e_1 \,\&\, e_2 \dots \&\, e_s)$ after s successful predictions. Assuming for simplicity that successive appraisals are all associated with the same value of ϵ, we can clearly write

$$p_n = \gamma^{(n)} \cdot \gamma^{(n-1)} \dots \gamma^{(1)} \cdot p_0$$

where $p_0 = x$ according to our previous notation

and
$$\gamma^{(s)} = \frac{1}{p_{s-1}(1-\epsilon)+\epsilon}$$

$$p_{s+1} = \gamma^{(s+1)} \cdot p_s$$

The solution of this recursion is by inspection or more simply by replacing ϵ by ϵ^n in the formula for p_1 (see (4) above). We obtain[1]

$$p_n = \frac{1}{1-\epsilon^n+\epsilon^n/x} \tag{6}$$

We can also ask what is the probability for the $(n+1)$th prediction e_{n+1} being correct if the theory has already made n successful predictions $e_1 \dots e_n$. Denoting this probability by $p(e_{n+1})$ we clearly obtain the result

$$p(e_{n+1}) = \epsilon + \frac{1-\epsilon}{1-\epsilon^n+\epsilon^n/x} \tag{7}$$

If ϵ is a small quantity (*i.e.* $\ll 1$) which will be the case if T is non-*ad hoc*$_2$ with respect to all the predictions, we can write the following formulae which will be perfectly satisfactory for the subsequent discussion

$$p_n \simeq \frac{1}{1+\epsilon^n/x} \tag{8}$$

---

[1] This result will be recognised as a special case of the general treatment given by Keynes in his [1921], pp. 235–6.

$$p(e_{n+1}) \simeq \epsilon + p_n \tag{9}$$

$$\gamma^{(n)} \simeq \frac{1}{\epsilon + p_{n-1}} \tag{10}$$

We note the following features

(1) The value of p_n depends entirely on the ratio ϵ^n/x.
For $\epsilon^n/x \gg 1$, $p_n \ll 1$ and for $\epsilon^n/x \ll 1$, $p_n \simeq 1$.
At the critical point $\epsilon^n = x$ we have $p_n \simeq 1/2$.
So if initially $x \ll \epsilon$ as n increases p_n will rise steeply as we reach the value $n = \ln x/\ln \varepsilon$.

(2) So long as $p_n \ll \epsilon$ we have $p(e_{n+1}) \simeq \epsilon$, but as p_n builds up towards unity so does $p(e_{n+1})$.

(3) So long as $p_{n-1} \ll \epsilon$, $\gamma^{(n)} \simeq 1/\epsilon$, but as p_{n-1} builds up towards unity the enhancement factor $\gamma^{(n)}$ also tends to unity.

To take a concrete case we illustrate in the accompanying figure p_n and $p(e_{n+1})$ as functions of n for the particular choice $x = 0{\cdot}01$, $\epsilon = 0{\cdot}1$.

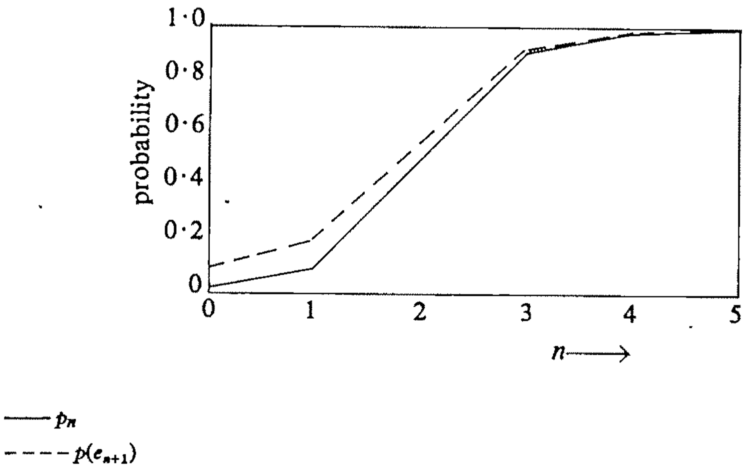


Figure 1.

3 QUANTITATIVE PREDICTIONS

We can apply this analysis to the important case of quantitative predictions.[1] Suppose a theory T predicts correctly an experimental result which is known to an accuracy of n significant figures. We assume as part of our background knowledge that the order of magnitude of the result is known, *i.e.* we disregard the prediction of zeros occurring before or after the significant figures in the experimental result. As a concrete illustration we cite the theoretical predictions by quantum electrodynamics of the anomaly in the hydrogen spectrum known as the Lamb shift and the anomaly in the magnetic moment of the electron. For example the latter is now known experimentally to be $(11596524\pm2)\times10^{-10}$ Bohr magnetons[1] whereas the theoretical prediction is $(11596524\pm6)\times10^{-10}$ Bohr magnetons.[2] We thus have remarkable agreement to seven significant figures. Clearly if we regard the prediction of each significant figure as an independent event then the appropriate value to take for ϵ is 0·1 since a false theory would have ten equal possibilities for filling in each digit. The question of what value to take for x is somewhat arbitrary. In agreement with Schaffner we do not follow a purely logical approach and set $x = 0$. For our purpose x reflects the scientist's confidence in the new theory T. One could argue that a scientist would not spend great efforts developing the consequences of a theory he did not believe in. By analogy with the situation in the Bayesian analysis of significance testing (see for example Redhead ([1974]) we could take $x = \frac{1}{2}$. Perhaps more realistically we should take x around 0·01 and adopt the sociological rule that unless a scientist has a one per cent level of confidence in the truth of his theory he would not seriously investigate it.[3] With this choice of parameters we see that the build-up of confidence in a theory which makes correct quantitative predictions, as the accuracy of the experiment increases, would be illustrated by the graph for p_n in the figure.[4] Of course $p(e_{n+1})$ is only given by our analysis so long as other factors which could potentially influence the results are known not to be significant. For a certain value of n this condition will fail. For example in the case of the anomalous magnetic moment of the electron the effect of hadronic couplings introduce theoretical uncertainties which would ultimately make the prediction of the theory unreliable.[5]

---

[1] The successful detailed quantitative predictions of a theory in respect of phenomena quite different from those which the theory was originally proposed to deal with have always attracted the attention of scientists. To take an example at random, in referring to his early work on the ground state of helium Hylleraas comments in his [1963] (p. 42) 'The end result of my calculation was . . . greatly admired and thought of as almost a proof of the validity of wave mechanics . . . in the strict numerical sense'.

CONCLUSION

The concept of a novel prediction plays a very important part in the way scientists assess their confidence in theories. Our analysis has shown how the intuitive appraisal by scientists in this respect can be understood if degrees of commitment are governed by Bayesian rationality constraints.

M. REDHEAD
Chelsea College

---

[1] Van Dyck *et al.* [1977].

[2] See value quoted in Calmet *et al.* [1977]. For a good account of the fluctuating agreement between theory and experiment the reviews by Lautrup, Peterman and de Raphael [1972] or Rich and Wesley [1972] may be consulted.

[3] We may refer to Shimony's concept of commitment to a theory (see his [1970], pp. 94–5). The degree of commitment measures the scientist's belief that the theory T belongs to the equivalence class of all theories which give the same true observational predictions within the domain of current experimentation and that the 'true' theory 'generalises' in some sense the concepts embodied in T. Commitment measures our belief, not that a theory is true, but that it points the way to the truth.

[4] It is easy to see that our confidence in a theory at a given level of accuracy for agreement between theory and experiment does not depend on the particular scale of notation used to express the result.

[5] According to Rich and Wesley ([1972]) the known hadronic contribution to the electron anomaly would affect the tenth significant figure.

REFERENCES

CALMET, J., NARISON, S., PERROTTET, M. and DE RAPHAEL, E. [1977]: 'The Anomalous Magnetic Moment of the Muon: A Review of the Theoretical Contributions', *Reviews of Modern Physics*, **49**, 21–9.

HYLLERAAS, E. A. [1963]: 'Reminiscences from Early Quantum Mechanics of Two-Electron Atoms', *Reviews of Modern Physics*, **35**, 421–31.

KEYNES, J. M. [1921]: *A Treatise on Probability*.

LAUTRUP, B. E., PETERMAN, A. and DE RAPHAEL, E. [1972]: 'Comparison between Theory and Experiment in Quantum Electrodynamics', *Physics Reports*, 3C, 195–259.

REDHEAD, M. L. G. [1974]: 'On Neyman's Paradox and the Theory of Statistical Tests', *The British Journal for the Philosophy of Science*, **25**, 265–71.

RICH, A. and WESLEY, J. C. [1972]: 'The Current Status of the Lepton g Factors', *Reviews of Modern Physics*, **44**, 250–83.

SCHAFFNER, K. F. [1974]: 'Einstein versus Lorentz: Research Programmes and the Logic of Comparative Theory Evaluation', *The British Journal for the Philosophy of Science*, **25**, 45–78.

SHIMONY, A. [1970]: 'Scientific Inference', in R. G. Colodny (*ed.*): *The Nature and Function of Scientific Theories*, 79–172.

VAN DYCK, Jr., R. S., SCHWINBERG, P. B. and DEHMELT, H. G. [1977]: 'Precise Measurements of Axial, Magnetron, Cyclotron, and Spin-Cyclotron-Beat Frequencies on an Isolated I-meV Electron', *Physical Review Letters*, **38**, 310–14.

WORRALL, J. [1978]: 'The Ways in which the Methodology of Scientific Research Programmes Improves on Popper's Methodology', in Radnitzsky and Andersson (*eds.*): *Progress and Rationality in Science*. Dordrecht: D. Reidel.

ZAHAR, E. G. [1973]: 'Why did Einstein's Programme Supersede Lorentz's?', *The British Journal for the Philosophy of Science*, **24**, 95–123 and 223–62.

ZAHAR, E. G. [1978]. 'Einstein's Debt to Lorentz: A Reply to Feyerabend and Miller', *The British Journal for the Philosophy of Science*, **29**, 49–60.

Brit. J. Phil. Sci. 29 (1978), 363–387 *Printed in Great Britain*

Review Articles

FAILURES IN CRITICISM: POPPER AND HIS COMMENTATORS*

In these two volumes on Sir Karl Popper the Schilpp series has excelled itself, at least in length. They contain his Intellectual Autobiography (182 pages), thirty-three 'descriptive and critical' essays by thirty-five authors (773 pages), Popper's replies (217 pages), a Bibliography of Popper's writings (86 pages), and a full Index. A thorough review would be impossible, and any review might seem superfluous. This will be an essential work of reference; libraries must have it, and anyone working on Popper's thought must use it. However, one may try to answer four related questions. How far does this collection succeed in illuminating the thought of the 'living philosopher' by expounding, defending, or criticising his views and by getting him to explain his intellectual history, to clear up misconceptions about his work, and either to accept or to rebut criticisms? Is it well-organised for this purpose, combining a thorough and searching study of Popper's work with economy and relevance? What particular qualities, what merits and what faults have the individual articles? How well, in the end, do Popper himself and his thought come out of this examination?

My general impression is that Popper's leading ideas and their history and background are adequately presented here; quite a number of 'legends' and misinterpretations are corrected; he shows plainly how he responds to some stock objections and to appeals for changes in his position, and on a very few small points he accepts correction. But the collection is not well-planned. It is unbalanced as well as being far too long. Some of Popper's main theses are stated over and over again, both by himself and by many of the other writers. His anti-inductivism is discussed repeatedly. But there is no critical discussion of his more recent doctrines about 'world 3' and evolutionary epistemology. The only defence of Plato against Popper is radically undermined by John Wild's failure to take any account of Popper's earlier reply to similar criticisms, and there is no defence at all of Hegel or Marx or of the claim of psychoanalysis to the status of a science. (Lakatos's argument that Newton's theory is equally unfalsifiable does not amount to a positive defence of Freud.) It may be that Popper's criticisms of all these adversaries are correct: but the other side has here been given no adequate hearing. Nor would an even longer collection have been needed to correct this: in my opinion nearly half the existing articles could—for different reasons—have been omitted with no great loss. Hilary Putnam's article (on corroboration) is a disaster: his main 'criticism' of Popper totally fails to take account of Popper's actual argument, as Popper has no difficulty in showing in his reply. Arnold Levison (on induction) also quite misses the point, taking Popper to have tried and failed to solve the traditional problem of induction, to give a positive justification for inductive conclusions and predictions. G. Schlesinger takes up what is for Popper a very minor matter, his discussion of 'self-reference' in certain paradoxes, and is more concerned with offering Schlesinger's own not very good solution of these paradoxes. I have mentioned the basic weakness of Wild's article on Plato. Peter Winch (on scientific method in the social sciences) makes what seem to me to be confused objections to the fact/value distinction, and insists on the importance of institutions; but Popper has neither denied nor ignored this.

---

* Review of Schilpp, Paul A. (*ed.*) [1974]: *The Philosophy of Karl Popper*. La Salle, Illinois: Open Court. The Library of Living Philosophers, volume XIV. $30.00. Two volumes, pp. xvi+xii+1323.

All these count mainly as critics. But some of the pro-Popper articles are no more to the purpose. That of J. W. N. Watkins is entitled 'The Unity of Popper's Thought', but it confessedly does not attempt to display this unity, since it does not deal with his moral, social, and political views. As an exposition of other parts of Popper's philosophy it would be useful in other contexts, but it is unnecessary here, where it largely repeats what Popper himself and others say elsewhere in this collection. To this exposition are added arguments for indeterminism which (as I shall argue) are weak and unsound. Alan E. Musgrave also mainly echoes the epistemological views that Popper states elsewhere in this collection, and takes rather unnecessary care to warn against a psychological reading of such terms as 'severe tests' and 'critical attitude'. Joseph Agassi suffers from an acute attack of ism-ism; his attempt to reclassify Popper's position throws no light on it, as Popper's reply shows. I see little point, at least in this context, in Czesław Lejewski's enterprise of thoroughly formalising Popper's suggested natural deduction procedures. Sir John Eccles, on 'The World of Objective Knowledge', expounds (with one small criticism) a view which is also treated in several other places, by Popper and others: the thorough criticism which would be needed to clarify this issue is quite lacking. Donald T. Campbell's article on 'Evolutionary Epistemology' is long and very badly written. It contains many quotations and references, and points to a number of thinkers who anticipated Popper's emphasis on trial and error. It ends with the claim that it has defended the compatibility of realism with an evolutionary perspective, but this defence is too murky to be effective. Another long article, by Eugene Freeman and Henryk Skolimowski, compares Popper and Peirce. Whatever intrinsic interest Peirce's views have, this report of them does not illuminate Popper's thought, and some of the remarks about his third world doctrine shed more darkness than light. For example, it is said that 'the objectivity of scientific knowledge is no longer sought in intersubjective criticisability . . . but in the autonomy of the third world entities' (p. 494), yet when we unpack this notion of autonomy, it rests largely on such intersubjective criticisability. Again, we are told that 'The third world doctrine . . . is an attempt to preserve the objectivity of science from the onslaughts of sociologism and psychologism' (p. 495). But such metaphysical elaboration would hardly be needed for this purpose, and Popper's reply denies that this doctrine was in any sense a reaction against Polanyi and Kuhn. H. B. Acton's article on 'Moral Futurism and the Ethics of Marxism' merely (as Acton says) 'call(s) attention to some features of Marx's ethics which Popper has not commented upon', but not in any way that constitutes a criticism of Popper's attack on Marxism. Tom Settle, writing on induction and probability, starts with yet another statement of how Popper has dissented from 'the popular view of induction'. Part I of his article could well have been dispensed with, though Part II contributes useful points both in defence and in discussion of Popper's propensity view of probability.

If all, or most, of these articles had been absent (and, consequently, the replies to them), room could easily have been found for the critical discussion that, as I noted, some important and controversial parts of Popper's theory have not received here, and the whole collection would have been less tediously repetitive and more relevant to its professed purpose. No doubt it is not in editors to command success, and it must be hard to reject or cut invited contributions, but Professor Schilpp seems this time to have had quite a run of bad luck, and not to have shown the judgment and determination that together might have made up for it.

It is pleasing to record, however, that there are some very good contributions. Grover Maxwell (on 'Corroboration without Demarcation') makes out a forceful case against falsifiability as a principle for the demarcation of science and in favour of a more positive confirmatory support for hypotheses than Popper allows. Herbert Feigl and Paul E. Meehl argue equally forcefully against Popper's reasons for adopting indeterminism (though not against indeterminism itself): determinism (or again quasi-determinism, with an element of randomness) is compatible with unpredictability in practice, with creative novelty, and with rationality. Adolf Grünbaum contributes a thorough discussion of Popper's view about 'the arrow of time', distinguishing anisotropy from 'direction' in the sense of something like a flow or movement, arguing that Popper's examples (such as the expanding wave) and certain extensions of them yield only the former, and that entropy increase in branch systems provides a further kind of anisotropy. In reply, Popper argues for 'direction' as well as anisotropy, but his argument is no more than an appeal to current belief and (!) ordinary language; he does nothing to show that physics could not work entirely with relative rather than absolute time distinctions (essentially McTaggart's B series rather than his A series). Alan Donagan contributes a careful and informed discussion of Popper's objections to 'historicism'.

A number of other contributions are somewhat less good but still useful. These include a characteristically vigorous piece by Imre Lakatos, arguing for a more complicated theory of demarcation and (like several others) for a more positive view of induction. Thomas S. Kuhn defends his own emphases with regard to the history of science in contrast with Popper's, but wastes space quibbling about Popper's calling theories that turn out to be false 'mistakes'. J. O. Wisdom offers a sensible comparison between Popper and Kuhn. William Kneale challenges Popper on demarcation, and though Popper's reply is that he has never made the claims which Kneale criticises, he himself misses part of Kneale's point, which is to stress the importance in science of theories whose content is historical and therefore singular rather than universal in logical form—*e.g.*, this was how biological (or cosmic) evolution took place—and which therefore do not involve, as universal hypotheses do, the immediate logical contrast between verification and falsification from which Popper's argument starts. That Popper is inclined to play down such theories is shown by his treatment of Darwinism as a 'metaphysical research programme' rather than a scientific theory (Autobiography, pp. 133–43): it 'does not really *predict* the evolution of variety' and 'therefore cannot really *explain* it'. This may be true of Darwinism as a set of general theses; yet the substantial historical claims about how evolution occurred surely constitute a scientific theory. Patrick Suppes, on probability in Quantum Mechanics, asks for a theorem that will show that propensities obey the probability calculus, analogous to those available for the Laplacean, frequency, and subjective interpretations of probability, and points to peculiarities of Quantum Mechanics considered as a statistical theory. In reply, Popper discusses the formal features of propensities, but seems not to give the most obvious answer to Suppes, that if propensities are measured by the frequencies they determine they must obey the same calculus as those frequencies. Brief challenges by W. V. Quine (on the relation of multiply quantified statements to falsification) and by Sir Alfred Ayer (on 'verisimilitude' and on experience as giving non-conclusive support for 'basic statements') evoke replies which clarify Popper's position. Victor Kraft provides historical background about Popper's relation to the Vienna Circle, and Sir Peter Medawar surveys the history of the hypothetico-deductive approach. J. Bronowski gives a personal report on the climate of opinion in the 1930s, and mainly sensible general comments on Popper's view of science, but quite wrongly suggests that the correspondence theory of truth does not apply to theories, apparently because they are not open to simple verification, and that the 'unifying' character of a good theory means that we need to combine a coherence theory of truth with the correspondence one. Henry Margenau's short article gives authoritative support for some of Popper's views about physics, while expressing a radically different theory of knowledge: Popper's reply merely notes both the agreements and the disagreements. Paul Bernays makes a mild plea for some kinds of rationality other than error elimination. Lord Boyle's enthusiastic assessment of *The Open Society and its Enemies* includes an acute criticism of Popper's belief that, despite the sharp distinction between facts and norms, there can be absolutely valid standards or objective values. Sir Ernst Gombrich, on 'The Logic of Vanity Fair', shows how changes of fashion, style, and taste in art may be explained better with the help of Popper's 'logic of situations'—essentially game theory models—than by reference to historicist principles.

Y. Bar-Hillel's article ('Popper's Theory of Corroboration') and Popper's acrimonious reply to it embarrass me. Bar-Hillel contrasts Carnap's approach with Popper's, presses certain criticisms, and asks for clarification on several specific questions. Popper takes it as based on radical misunderstanding and as asking for repetition of points that he has already made abundantly clear, and he explicitly blames Bar-Hillel for starting the controversy between him and Carnap and for creating 'most, if not all of "the disturbing heat" '. I can make no comment on this exchange, being ignorant of the personal history that lies behind it.

My assessments of these thirty-three articles have been stated somewhat dogmatically. It would take far too long to defend them all, but I shall go further into a small number of representative issues: the demand (pressed by Maxwell, Lakatos, Bronowski, Ayer, and others) for something more like a positive theory of inductive support than Popper allows; indeterminism; world 3; and objective standards of value.

INDUCTIVE SUPPORT

Popper has repeatedly criticised what he calls both 'the commonsense theory of knowledge' and 'the bucket theory of the mind'—that the mind is initially empty, sensory material is fed in, at first in the form of unadulterated elements of information, passively received, that we can be certain about these as long as they are kept unadulterated, but that we also need knowledge which goes beyond these elements, and that we get it by associations of these elements (through repeated occurrence) which yield beliefs which may be true, but may be false. What is fundamentally wrong with this is that it sees us as engaged in a search for certainty. In fact we have no pure data: we have dispositions, partly innate, partly learned, for decoding sensory messages which yield an appearance of something 'given'. But this and all further knowledge is really acquired by processes of trial and error-elimination. Hence there is no such thing as induction (as Hume, for example, saw it), and therefore no need to justify it. Thus Popper is happy to accept the conclusion that 'there *are* no such things as good positive reasons' (for relying on a predictive consequence of a well-tested theory) (p. 1043).

Most of what Popper says in this vein is correct and important. We cannot achieve certainty, and it is both pointless and misleading to strive after it. Observation is 'theory-impregnated' and knowledge grows not by Humean association and induction but the framing and testing of hypotheses; much good work in science is speculative rather than cautious, and it is anti-scientific to protect one's hypotheses from falsification. (Though it is more disputable whether the contrary view is the commonsense or even the dominant empiricist one: even Locke, for example, insisted that 'the ideas we receive by sensation are often in grown people altered by the judgment, without our taking notice of it' (*Essay* II, ix, 8), that is, there is automatic and unconscious (but learned) interpretation of sensory input.) But to recognise this is fully compatible with the following claims. We believe that there is a real, objective, world, some aspects of which are not too different from the way in which we naively (but in virtue of both innate and learned interpretative dispositions) see them. We make extensive extrapolations from observed to unobserved (including future) but analogous cases, especially of persistences and of what we take to be causal processes. We conjecture, more tentatively, that well-tested deeper explanatory theories contain some approximations to the truth and that what we can recognise as the advance of science in general brings us closer to the truth. We therefore suppose that predictions based on such theories will often turn out to be correct. Though all these beliefs are fallible, we can sensibly raise the question whether there is any positive rational support for them. If there is any such support, the whole range of our experiences will play an indispensable role in it, despite the fact that their content is not 'unadulterated'. There is no absurdity in asking whether and how our relatively basic realistic judgments are supported by our experiences (holistically rather than singly), also whether and how these judgments support extrapolations of persistences and causal sequences, and the view of well-tested theories as (often) getting nearer to the truth. These questions still arise even though there are no such inductive processes as Hume described and no passive reception of guaranteed information, and there is no reason to rule out from the start the possibility of positive answers to them. That is, there could be a thoroughly fallibilist empiricism, incorporating some of Popper's doctrines and (as here) of his terminology, which yet includes a theory of positive support. Indeed Popper supplies a part of this theory: he frequently asserts realism, and occasionally argues for it (*e.g.* in his [1972], pp. 33–44, 64–5), though he thinks that anti-realist views hardly deserve to be taken seriously. Of the arguments he mentions some are general appeals to common sense and the presuppositions of language, and some rebut arguments for idealism or subjectivism, but one is that realism is the best (or rather the only sensible) hypothesis. Hypothesis to explain what? Surely the whole range of our experiences, including those that we take to convey to us recordings made by automatic machines. In so far as the 'crossbearing' principle ([1972], p. 43) is relevant as an argument for realism, it must be used in a context in which realism is as yet only one among rival interpretations of something not fully committed to realism, that is, our experiences considered simply as such. Is it out of the question that there should be a positive argument also in favour of causal extrapolations? It is true that such extrapolation, in contrast with deeper explanation, is not of scientific interest: it is commonly taken for granted. (It is implicit in the very search for explanatory theories of universal form. In seeing the choice as being between this theory and that, we assume that there are universal theories to be found, that whatever is the correct explanation for some range of observed behaviour will hold in all relevantly similar cases.) But whether there is any rational support for this taking of it for granted is still of *philosophical* interest. Similarly, is it out of the question that there should be a positive argument in favour of what Popper calls the verisimilitude of a well-tested theory, and of the likelihood of increasing verisimilitude as one theory is replaced by another because it would explain more, or better, or because it survives tests which the former fails? It is for this that Lakatos asks when he says (p. 254) that it is not enough to define progress (in terms of verisimilitude), we have to recognise progress, and that this can be done by 'an inductive principle which connects . . . verisimilitude with corroboration, which reinterprets the rules of the "scientific game" as a—conjectural—theory about the *signs* of . . . growing verisimilitude'. In reply, Popper notes (p. 1011) that he has said (in his [1963]) that 'we may *guess* that the better corroborated theory is also one that is nearer the truth'. But is this a reasonable guess, and if so why? Popper is reluctant to say. He still denies that if one lives up to his methodological appraisals 'one has a *better chance* to get nearer to the Truth than otherwise'. I see no good reason for Popper's refusal to consider a positive theory here. He seems to associate this suggestion with the various aspects of inductivism (or the commonsense theory) which he has always rightly opposed; yet there is no necessary connection between them.

It is in this context that we can consider Maxwell's suggested contingent confirmation theory stated in terms of the (not precisely measurable) probability of a thing's having a certain verisimilitude, itself supported by the theory that (as a result of natural selection) we have 'innate capacities to acquire abilities to make *some* guesses with prior probabilities appreciably greater than zero as well as abilities to order competing hypotheses . . .' (p. 313), while this theory of innate capacities is itself vindicated as the best explanation of the fact that the species has survived as well as it has and of its technological achievements (p. 315). This is at least an interesting suggestion for a positive theory which is justificatory but still fallibilist. It does not transgress the warning Popper gives in his reply to Ayer: 'he who wishes to solve the problem of induction must beware of trying to prove too much'—because there are logically possible worlds in which the acquiring of knowledge would be impossible.

Popper allows that Maxwell's argument is interesting and plausible and has even 'a kernel of truth'. Yet in the end he will have none of it: we should reject all explanations of our success, for they all prove too much (pp. 1040–1). He relies on the Humean point that any such evolutionary account would explain only our *success so far*, and would yield no positive reasons for beliefs about the future. All he will allow is a 'metaphysical faith' in the existence of regularities and in our having discovered some of them. 'But the (objective) content of this subjective faith is something to marvel at rather than to explain.' Yet there is surely a positive ground for at least limited extrapolations: even if regularities were going to fail, it would at any given moment be improbable that the cut-off should be here and now.

And, curiously, Popper does admit that there is something to be said for the argument that 'if a theory passes many varied tests it is highly improbable that this is due to an accident; highly improbable therefore that the theory is miles from the truth' (p. 1041), and he does say (with what he himself calls 'a "whiff" of inductivism') that this sort of consideration may make it probable that one theory has greater verisimilitude than certain rivals (pp. 1192–3). This argument is stated in his [1972] (pp. 101–3), where he says 'I do not think that much can be said against this argument, even though I should dislike its being developed into yet another theory of induction'. But is this dislike any more than an emotional echo of his hostility to theories that have tried to support induction in a different sense? I should agree, indeed, that not much development is needed: purely technical elaboration of the key point would be both tedious and unnecessary. Yet there is in an important sense a theory of induction here, and a sound one. This yields what Lakatos was asking for, and, contrary to what Popper says in his reply to Lakatos, it shows how one has a better chance to get nearer to the truth. And taken along with the analogous support for at least limited extrapolations and the arguments for realism as a hypothesis to explain experiences, it shows that the fallibilist empiricism sketched above can incorporate a theory of positive support.

INDETERMINISM

Watkins distinguishes metaphysical determinism, the thesis that all events are precisely predetermined by earlier causes, so that the future is fixed by causes that have already occurred, from scientific determinism, the thesis that there is no limit in principle to the precision with which this fixed future may be foreknown. There are strong arguments against scientific determinism, including one of Popper's and others mentioned by Feigl and Meehl (pp. 521–5), and hardly anyone would now want to defend it. The controversial issue is that of metaphysical determinism. Popper also stresses a related but distinct issue, whether the physical world ('world 1') is causally closed. World 1 might be thus closed even if it were somewhat indeterministic; equally it might be open even if the universe as a whole (including, if they are real, worlds 2 and 3) were deterministic: it might be that every event has some antecedent sufficient cause, but that the causally sufficient conditions for world 1 events sometimes include items in worlds 2 and 3.

Let me begin with some of the arguments about metaphysical determinism. It is agreed that apparently deterministic macrotheories can be explained on the basis of indeterministic microtheories; but Popper denies that the opposite is possible (p. 75). This goes against the widespread view that there can be a deterministic account of the apparently indeterministic behaviour of gambling devices. In support of this denial, Popper refers to 'some very interesting arguments of Landé's' (*e.g.* in *Mind*, 1958), and Watkins presents and endorses the key argument. Suppose that balls roll down a chute onto an adjustable blade from which they fall to right or left—initially, say, about half to each side. Someone who saw this process as deterministic would explain this apparently random statistical result (with, say, one thousand balls) as due to 'little unobserved asymmetries' which affect the individual balls differently. But, says Watkins, following Landé, universal determinism implies that at any chosen earlier date there was a corresponding sequence of world-states, of which half rigidly predetermined the later falling of balls to the left, and half predetermined the later falling of balls to the right. 'But *why* . . . should there *then* have been approximately *five hundred* left-determining states? . . . Why should the world *then* have been so nicely geared to the statistical requirements of our probabilistic setup today?' (p. 379).

But this is an absurdly prejudicial question. All that we need to suppose is that just as now there is a scatter of input conditions affecting the exact positions of the balls in the chute, slight irregularities in the balls, *etc.*, so *n* years ago there was a corresponding scatter in the no doubt very complex sets of causal ancestors of these conditions. There is no need for them to be 'nicely geared' to the present setup. All we need to postulate, *n* years ago, is a scatter of conditions that would determine the present scatter of ball-inputs. Any gearing of this to the fifty per cent statistics is done by the central positioning of the blade in relation to the correspondingly scattered ball-outputs from the chute.

In reply to what amounts roughly to this answer, Watkins says 'Its plausibility wanes when we turn to randomising setups that give unequal probabilities to the possible outcomes'. If we adjust the blade so that about three-quarters of the balls fall to the left 'why should the original chaos have thrown up a sequence containing about three times more left-determining than right-determining states?' Reply: simply because it was a 'chaos', that is, a scattered sequence of sets of conditions which causally produced the present scattered ball-outputs from the chute. Of course when the blade is moved across this spray of balls, it divides it differently. Watkins also suggests that this answer merely backdates 'ontological randomness'. But this obscures the crucial difference between 'randomness'—that is, scatter—in initial conditions and 'randomness'—that is, indeterminism—in laws of working. The determinist has no desire to deny the former, and Watkins's argument is powerless to make him admit the latter *at any stage*. Watkins adds the further absurdity that this answer 'seems to suggest *that we can control the past*: for we now seem free to . . . adjust the proportion of left-determining . . . states in the remote past' (p. 380). Of course not. If we are free to adjust the blade, we are free to make more or fewer of those already fixed past states left-determining. But this no more controls the past than does my swatting or not swatting a fly now, thereby controlling the number of swatted flies this fly's grandfather was to be the grandfather of.

Of course these replies do nothing to prove or confirm determinism. My purpose is only to point out that this Landé–Watkins argument is powerless against determinism, and does not support Popper's claim that apparently indeterministic macrotheories cannot have a deterministic basis, if this means a basis in which all the relevant laws of working are deterministic.

I shall not repeat what seem to me to be the largely sound arguments of Feigl and Meehl—*e.g.* that 'plastic control' and 'cloudiness' can easily be fitted into a deterministic scheme—but turn to Popper's reply (pp. 1072–8). Despite their explicit disclaimer (pp. 557–8) he seems to take them to have argued *for* determinism, whereas their thesis is only that determinism is compatible with various things with which Popper and others have seen it as incompatible. He does not argue here either against this thesis or against determinism itself. He refers, indeed, to the argument (in 'Of Clouds and Clocks') about the vagueness of psychological determinism; but whatever vagueness there is in any psychological laws that we might state, this does not inject any vagueness into the general determinist thesis that *there are* laws, some of which we shall perhaps never be able to state precisely, and some of which may involve psychological antecedents, in relation to which all events have antecedent sufficient causes. But he is mainly concerned to deny that world 1 is causally closed. Feigl and Meehl make out a case for this at least as a possibility. Their main point is that rational thought can still be causally efficacious, even if the physical system is closed, provided that the rational thought, and the dispositions to think rationally, are embodied somehow in microstructures that are in principle physically describable. As I would put it, if the relation of 'embodiment' between the rational thought or dispositions and the microstructure is sufficiently tight, these two, though distinguishable, do not compete with one another for a causal role. To insist, as Popper does, on the efficacy of reasons is no reply to a thesis which endorses this.

Popper thinks that the only objection to his thesis (of the openness of world 1) would be 'the denial of the real existence of world 3' (p. 1074). But Feigl and Meehl are not physical monists, since they admit both distinct mental properties, including rationality, and 'logical categories' (p. 548). They are not saying 'What really exist are not reasons or arguments or objections, but physical entities . . . thus there is no world 3' (Popper, p. 1074), but rather 'There are reasons and arguments and objections and they are physically embodied'. Though they do not speak in terms of 'world 3', they explicitly accept everything that could (as I shall argue) be defensibly meant by the assertion of its real existence. Against their real theses, therefore, Popper has given no argument.

I hope it will be understood that I have not argued for determinism any more than have Feigl and Meehl. The basic physical laws *may* be probabilistic, and then realism will, as Watkins says (p. 377), commit us to indeterminism. Whether they are so is an empirical question, not to be settled either way by general arguments. But even if the basic laws are probabilistic, I still have reservations about the interpretation of them in terms of propensities, which I have expressed elsewhere and will not repeat here.[1]

WORLD 3

I have complained that the third world doctrine is not criticised in the present work. One problem is to decide what metaphysical claims, if any, are being made. Popper is a mind–body dualist in that he recognises mental states as distinct from physical states, though not mental (or material) substances (*e.g.*

---

[1] See my [1973], pp. 179–87, 232–3, and [1975], ch. 9.

in his [1972], pp. 107, 231). We might expect that world 3 items would similarly be a distinct class of states or properties, rather than substances. The inmates of this world are said to include '*objective contents of thought*, especially of scientific and poetic thoughts and works of art', '*theoretical systems* . . . *problems* and *problem situations* . . . *critical arguments* . . . *the state of a discussion* . . . *scientific knowledge*', 'intelligibles . . . *ideas in the objective sense* . . . possible objects of thought' such as the sequence of natural numbers ([1972], pp. 106–8, 154, 160), and 'statements in themselves' which stand in logical relations to one another but cannot stand in psychological relations (Autobiography, p. 144). However, these lists contain items of at least two distinct sorts. There are the statements, theories, arguments, problems and so on 'in themselves', which are indeed intelligibles or possible objects of thought; when they are actual objects of thought, they could also be called contents. But there are also such things as problem situations, the state of discussion, scientific knowledge as it is at a particular time. The first group could be called logical third world items; they are timeless, and it is they that stand in logical relations, while some of them also contain logical relations between their parts. The second group could be called, for contrast, historical third world items; it is they that may change and grow. Popper considers the suggestion that the whole third world is timeless, but rejects it. 'Not only does it fail to solve the problem of the ontological status of the third world, but it makes this problem insoluble from a rational point of view.' It fails to explain whether we can act upon third world objects, and, if not, how they can act upon us. Rather, the third world is '*essentially the product of the human mind*' and 'has a history' (Autobiography, pp. 148–9). That is, he seems to confine world 3 to what I called the historical items. But surely it is not a matter of choice whether to think of a logical or of a historical world 3: items of both sorts are needed. A 'statement in itself' does not have a history, and while a 'theory in itself'—say the atomic theory—may, what this means is that different forms of atomic theory have been successively formulated and discussed, these formulations and discussions being causally related, but each specific form is timeless. Popper suggests that the timeless view of the third world 'is based . . . upon the misunderstanding that the status of the *logical relations between* third-world objects must be shared by these objects'. Does not this mean that those relations, at least, are timeless inmates of this world? Popper says that his view 'makes the reality of the third world understandable. It is as real as other human products, as real as a coding system—a language; as real as (or perhaps even more real than) a social institution . . .'; and he suggests that 'we may include in the third world in a more general sense all the products of the human mind, such as tools, institutions, and works of art' (pp. 148–9).

It would be pedantic to object to such generalisation, or to the inclusion in one 'world' of such different things as the logical items (possible objects of thought), the historical items (traditions, institutions, states of discussion, public knowledge, etc.), and tools and works of art considered as such (most of which will also be physical objects, and as such will belong to world 1). Still, these different categories should be distinguished, and it is plainly in different senses that they act on us (or 'through us' on world 1), and that we produce or act upon them. We make an axe or a statue; we formulate a theory or a problem, and again a problem may arise within some set of ideas we have formulated; an institution or a tradition or a state of discussion is constituted by complex relations between thinking beings. The axe helps us to fell trees; our grasp of the theory enables us to do new things; a tradition or an institution may influence or control a person's views and actions. Items in different categories will also be autonomous in different senses. A logical item has whatever logical features and relations it has or involves independently of anyone's thinking about it. A work of music exists in many separate performances, and therefore independently of any one performance; between performances it exists in the score, the memories of musicians, and their stored capacities to reproduce it; a body of scientific knowledge may exist not in any one mind but in the sum of the thoughts and capacities of many minds, any one of which may be dispensable, and also in physical records which *could* be interpreted by minds; a state of discussion is constituted partly by the logical features of the topic and partly by relations between minds, but again because many minds are involved any one may be dispensable. Popper concedes that what is causally effective is not, *e.g.*, theories in themselves but, say, books (which yet have the effects they do only because of their theory-content) and again our grasp of those theories (p. 147). The fact that the theory itself matters in that one person may misunderstand it and another may correct him still leaves it true that what is effective is someone's understanding (or misunderstanding) of that theory. It seems, then, that Popper implicitly concedes that worlds 1 and 2 together are causally closed—this being entirely compatible with the efficacy and importance of world 3 as he can explain and stress them—and this at least leaves open the possibility that world 1 is causally closed in the way that Feigl and Meehl suggest, reluctant though Popper is to admit this. When the various claims are sorted out, the pluralist metaphysics loses much of its dramatic force and novelty.

Popper sees as opponents of the third world thesis those thinkers who are 'interested in our subjective beliefs, and their basis or origin', and who, while they of course admit that there are problems, theories, books, and the like, see them essentially as 'symbolic or linguistic *expressions* of subjective mental states' ([1972], p. 107). Against them he is insisting on the importance of the logical relations, also on that of intersubjectivity, of the participation of many thinkers in critical discussion, and of the publicity of formulation that makes such discussion possible, and on that of traditions, especially the scientific tradition. But all of these undeniably correct emphases can stand without the grandiose metaphysics that his language suggests. Yet it is not clear how much is claimed when he says that the world 'consists of three ontologically distinct sub-worlds' ([1972], p. 154); perhaps this is only a picturesque way of presenting the claims and emphases that I, for one, would accept. These are some of the issues that might have been clarified by the critical discussion of world 3 the lack of which, in this collection, I am deploring.

OBJECTIVE VALUE

Lord Boyle thinks that Popper's belief in 'absolutely right or valid proposals in the realm of standards', analogous with absolute truth in the realm of facts, is inconsistent with his dualism of facts on the one hand and norms and decisions on the other (p. 851). Popper, on the contrary, thinks that Lord Boyle's rejection of this belief is inconsistent with that same dualism (pp. 1155–9), because he equates Boyle's view with 'moral relativism'. Now there is a view, often called moral relativism, to the effect that whatever any man (or society, or class, *etc.*) thinks right or calls right is (objectively) right for him (or it). This view is completely untenable: it first assumes that nothing is objectively right, so that all we have are the subjective callings right (by this or that man, society, *etc.*) and then moves, inconsistently, to the conclusion that what each calls right is objectively right for him. Popper is certainly justified in rejecting this view and in pointing out that (among its other faults) it would deny that we are fallible. But this view is utterly different from the consistent denial of the objectivity of values which Boyle endorses, following Ayer. Elsewhere I have argued that there is nothing objectively prescriptive, nothing which in itself directs us to pursue one end rather than another.[1] If so, the Ayer–Boyle view is ontologically correct, though it would not be correct as an account of the ordinary meaning of moral terms, because a strong popular and traditional belief in objective prescriptivity has been built into that ordinary meaning. Popper at least seems to be opposing this view, as well as the incoherent moral relativism, when he says that 'although norms and standards are our own creation, *some can be better and some can be worse*', that 'there are differences in worth, and that we *ought* to choose the best . . .' (He seems to hold that there is absolute rightness, though there is no criterion of absolute rightness.) But how are some norms *better*? Of course they may be better or worse in relation to some purpose which they help, or do not help, to promote. But then the question is whether one purpose is absolutely, objectively, better than another. To say this would be to erect a standard for standards, and to say (implausibly) that though first-order standards are our own creation, this second-order standard is not. Yet Popper admits 'the true insight that there cannot be any normative science of morals'. (It is also worth noting that a consistent denial of objective values does not commit us to another variety of 'moral relativism', the subjective neutrality that endorses all other subjective valuations alike, any more than to the incoherent variety stated above.) I am left with the impression that Popper's position on this issue is imperfectly clarified, partly because he thinks, mistakenly, that some kind of moral relativism is the only alternative to the recognition of objective standards, but also, I suspect, partly because the strength of his own adherence to certain values leads him to objectify them.

Finally, how well do Popper and his philosophy survive the scrutiny provided or provoked by this collection? He has a many-faceted but systematic and (on the whole) coherent philosophical position. Over many years he has argued vigorously and effectively, often in opposition to currently dominant fashions, and many of his once unpopular views have won a considerable measure of acceptance. On a number of central topics, such as realism, fallibilism, the hypothetico-deductive procedure, the importance of severe tests (and therefore of testability), of problems, and of public criticism he has simply been right when other influential thinkers have been wrong. In these volumes he wins most (though, as I have shown, not all) of the debates, and not only because the arrangement gives him the last word. Too many of his critics have overlooked the subtlety of his thinking, the fact that he has allowed for complications which they advance as objections. On the other hand, there seems to be a real conflict between the concessions that he correctly makes in a few places to inductivism (in one important sense) and the main drift of what he has repeatedly said about induction, and there may be a similar conflict between the firm commitments of his third world doctrine and the dramatic impression it conveys. On some other topics he seems unwarrantably inflexible. But my main conclusion is that Popper's controversial and challenging views call for more searching criticism than this collection has given them.

---

[1] See my [1977], ch. 1.

J. L. MACKIE
University College, Oxford

REFERENCES

MACKIE, J. L. [1973]: *Truth, Probability and Paradox.* Oxford: Clarendon Press.
MACKIE, J. L. [1975]: *The Cement of the Universe.* Oxford: Clarendon Press.
MACKIE, J. L. [1977]: *Ethics: Inventing Right and Wrong.* Harmondsworth: Penguin Books.
POPPER, K. R. [1963]: *Conjectures and Refutations.* London: Routledge and Kegan Paul.
POPPER, K. R. [1972]: *Objective Knowledge.* Oxford: Clarendon Press.

ON A NEW THEORY OF EPISTEMIC PROBABILITY *

Glenn Shafer's recent book offers an original and challenging alternative to the Bayesian theory of epistemic probability.

Shafer has two principal objections to Bayesian inference. In the first place he rejects as too restrictive the orthodox requirement that degrees of belief should be *additive*. In his view this has the consequence, amongst others, that the Bayesian theory is unable to represent complete ignorance. 'It does not allow one to withhold belief from a proposition without according that belief to the negation of the proposition' (p. 23). In the second place he rejects the Bayesian rule of *conditioning* as too limited a procedure for revising prior opinions in the light of new evidence. The rule is criticised for requiring that the new evidence be expressible as a proposition, that the proposition be one for which the prior degree of belief is already established, and that the proposition be known with certainty. For Shafer, a body of evidence is 'a segment of our experience and natural knowledge' (p. 175) and cannot, in general and without distortion, be conceptualised in the way required. In place of the Bayesian rule of conditioning Shafer advocates a rule of 'combination', originally proposed by A. P. Dempster [1967], whose purpose is to combine two or more belief functions based on distinct bodies of evidence into a single belief function based on the combined evidence. I shall now look in more detail at these characteristic features of Shafer's theory.

If degrees of belief are interpreted by the normal means of betting rates, the condition of additivity follows essentially from the conventions ruling the betting scheme. Thus, strictly speaking, it would be more appropriate to question the ***usefulness*** of the concept established by these conventions than the ***validity*** of the condition itself. The crucial feature of the betting scheme normally adopted is that the subject is required to choose a rate at which he is equally prepared to bet on or against the event in question, that is to say he is prepared to take either side of the bet or, in short, that the bet be ***fair*** (for him). If this requirement is relaxed, however, one can imagine situations in which the subject would be willing to bet at any rate less than some number $P_*(E)$, say, ***on*** the event E in question and at any rate greater than $P^*(E)$, say, ***against*** the event E. If $P_*(E)$ is strictly less than $P^*(E)$, there is no rate at which he is indifferent between the two sides of the bet. Assuming that two such real-valued functions P_*, P^* are defined in this way over the space of events under consideration, it follows from the definitions, for example, that for any events A and B

---

* Review of Shafer, Glenn [1976]: *A Mathematical Theory of Evidence.* Princeton and London: Princeton University Press. Cloth, £11.00; Paper, £5.60. Pp. xiii+297.

$$
\begin{aligned}
&P_*(A)+P^*(\overline{A}) = 1 \\
&P^*(A \cup B) \leqslant P^*(A)+P^*(B) \quad \text{if } A \cap B = \varnothing \\
&P_*(A \cup B) \geqslant P_*(A)+P_*(B) \quad \text{if } A \cap B = \varnothing .
\end{aligned}
$$

It can be shown then that a necessary and sufficient condition for the subject's beliefs to be coherent, in the sense that he can never be obliged to accept a bet from which he is bound to lose whatever happens, is that P_*, P^* should be the lower and upper envelopes, respectively, of a family of additive belief functions defined over the space of events in question.[1] More precisely, there should exist a family $\mathscr{P}$ of additive belief functions such that, for any event E,

$$
\begin{aligned}
P^*(E) &= \sup\{P(E) : P \in \mathscr{P}\} \\
P_*(E) &= \inf\{P(E) : P \in \mathscr{P}\}.
\end{aligned}
$$

In view of the stated relation between upper and lower probabilities it suffices to deal explicitly with only one of such a pair of functions, provided the space over which they are defined is closed under negation. Shafer chooses, for the greater part, to deal with the lower probabilities which he calls 'degrees of belief' and denotes by means of the function symbol '*Bel*'. It should be stressed, however, that Shafer does not choose to identify his degrees of belief with lower betting rates nor, indeed, to give them any other operational interpretation. As we shall see, this sometimes makes it hard to assess the merits of his proposals. Furthermore, Shafer restricts attention to a proper subset of the functions representing coherent lower betting rates. This subset can be defined as follows.

For an ***additive*** belief function (probability distribution) it is clear that, for any events $A_1, \ldots, A_n$, we have

$$
P(A_1 \cup \ldots \cup A_n) = \sum_i P(A_i) - \sum_{i<j} P(A_i \cap A_j) + \ldots + (-1)^{n+1} P(A_1 \cap \ldots \cap A_n).
$$

The condition characterising Shafer's belief functions, assumed to be normed and non-negative, is that the degree of belief $Bel(A_1 \cup \ldots \cup A_n)$ assigned to any disjunction $A_1 \cup \ldots \cup A_n$ should be ***greater than or equal to*** the corresponding sum on the righthand side of the above expression.

---

[1] See Smith [1961] for the case of a finite space or, more generally, Williams [1976]. For a corresponding treatment of conditional probabilities see Williams [1975].

The simplest example of such a function is the *vacuous* belief function representing an absence of any relevant evidence. Only the sure event has any support:

$$Bel(\Theta) = 1$$
$$Bel(A) = 0 \quad \text{otherwise.}$$

Another simple case is one in which 'the evidence points precisely and unambiguously to a single non-empty subset A of Θ' (p. 75). If p is the degree of support for A, this situation is represented by

$$Bel(B) = \begin{cases} 1 & \text{if } B = \Theta \\ p & \text{if } A \subseteq B \text{ and } B \neq \Theta \\ 0 & \text{otherwise.} \end{cases}$$

Such a function is said to be a ***simple support function focused on A***. Simple support functions are important inasmuch as the class of all belief functions can be obtained from the simple support functions by the operations of combination (to be described below), taking limits and forming restrictions.

There is an equivalent way of characterising belief functions needed for describing the rule of combination. Shafer considers only discrete spaces, more particularly the fields of all subsets of one or another finite set Θ called the 'frame of discernment'. For such a field, as is well known, an *additive* belief function is uniquely determined by an associated point function expressing the probabilities $p(\theta)$, say, of singletons. In fact we have for any subset A of Θ

$$P(A) = \sum_{\theta \in A} p(\theta).$$

Shafer's belief functions are similarly determined by an auxiliary function but, in this more general case, by a set function defined on the subsets of Θ. It is called a *basic probability assignment*, denoted by 'm', and required to satisfy:

$$m(\emptyset) = 0 \tag{1}$$
$$\sum_{A \subseteq \Theta} m(A) = 1, \tag{2}$$

where the summation extends over all distinct subsets of Θ. As Shafer explains (p. 38): 'The quantity $m(A)$ measures the belief that one commits exactly to A, not the total belief that one commits to A. To obtain the measure of the total belief committed to A, one must add to $m(A)$ the quantities $m(B)$ for all proper subsets B of A:

$$Bel\,(A) = \sum_{B \subseteq A} m(B).\text{'}$$

This relation establishes a one–one correspondence between belief functions and basic probability assignments.

Let us now look at the rule of combination. Its purpose may be illustrated by the following examples. A criminal defendant has an alibi supplied by a friend. It provides evidence, though perhaps not much, for his innocence. There is strong circumstantial evidence, however, of his guilt. What is the combined effect of these two bodies of evidence? Again, suppose that a shop has been burgled. There is evidence that the burglar was left-handed. There is also evidence that the burglary was an inside job. What support does the combined evidence provide for the various possibilities: left-handed insider, left-handed outsider, *etc*? The purpose of the rule is to answer such questions, that is to say to combine belief functions based on distinct bodies of evidence into a single belief function representing the effect of the combined evidence. Its application in particular cases can be a delicate matter for reasons that we shall have to consider later. For the moment it is enough to explain the mathematical aspects of the process of combination.

Suppose given, therefore, two belief functions Bel_1 and Bel_2 defined on the same field of events. Let their corresponding basic probability assignments be m_1 and m_2. The *orthogonal sum* of Bel_1 and Bel_2, denoted by $Bel_1 \oplus Bel_2$, is defined to correspond to the basic probability assignment

$$m(\varnothing) = 0 \tag{3}$$

$$m(A) = K \sum_{A=B\cap C} m_1(B)m_2(C) \quad \text{otherwise,} \tag{4}$$

where K is a renormalising constant determined by (2). If every term in the summation vanishes for every non-empty A, there is no such number and $Bel_1 \oplus Bel_2$ is said not to exist. But this only happens if Bel_1 and Bel_2 'flatly contradict each other', *i.e.* there exists a subset A of Θ such that $Bel_1(A) = Bel_2(\overline{A}) = 1$.

Since additive (Bayesian) belief functions are also belief functions in the present sense, we can consider, as an illustration, the combination of two such functions, P_1 and P_2 say. Since basic probability assignments for additive belief functions vanish except on singletons, the orthogonal sum $P = P_1 \oplus P_2$ is given, when it exists, by

$$P(A) = K \sum_{\theta\in A} P_1(\{\theta\})P_2(\{\theta\})$$

where K is again a renormalising constant. Thus the probabilities of singletons are multiplied together, with suitable renormalization, and probabilities elsewhere are determined by additivity.

As another illustration, suppose that initial evidence determines a prior belief function Bel defined over the subsets of Θ. Suppose that subsequent evidence establishes a particular subset B of Θ with certainty. How does the new evidence affect the prior beliefs? Shafer takes such evidence to be expressed by the belief function

$$Bel_B(A) = \begin{cases} 1 & \text{if } B \subseteq A \\ 0 & \text{otherwise.} \end{cases}$$

The posterior belief function, expressing the combined effect of the initial and subsequent evidence, is understood to be given by the orthogonal sum $Bel \oplus Bel_B$. Denoting this by $Bel(\ |B)$, by analogy with the Bayesian case, it follows that the associated upper probability function $P^*(\ |B)$ is given, for every subset A of Θ, by

$$P^*(A|B) = \frac{P^*(A\cap B)}{P^*(B)}$$

where P^* is the upper probability function for *Bel*:

$$P^*(A) = 1 - Bel(\bar{A}) \text{ for every } A \subseteq \Theta.$$

Having now outlined the basic tools of Shafer's theory it is possible to make a start on the task of assessment. The first questions I want to raise are these. (*i*) What is meant by 'degree of belief' and how might an individual determine his degrees of belief in a particular case? (*ii*) For what reasons are degrees of belief required to satisfy the conditions imposed? (I take it to be generally known that Bayesians have answers to these questions.) All that can be found in Shafer's book in answer to the first question is this (p. 20):

> I do not pretend that there exists an objective relation between given evidence and a given proposition that determines a precise numerical degree of support. Nor do I pretend that an actual human being's state of mind with respect to a proposition can ever be described by a precise real number called his degree of belief, nor even that it can ever determine such a number. Rather, I merely suppose that an individual can make a judgment. Having surveyed the sometimes vague and sometimes confused perception and understanding that constitutes a given body of evidence, he can announce a number that represents the degree to which he judges that evidence to support a given proposition and, hence, the degree of belief he wishes to accord the proposition.

Although Shafer has particular suggestions to make in the case of statistical inference, which I shall discuss below, he has no general advice to the individual what degree to 'announce', nor does he explain what practical difference it would make announcing one degree rather than another. This makes it difficult to see how any restrictions on degrees of belief could be justified. Concerning this second question, he says of the rules imposed (p. 6):

> I do not pretend that an individual would be 'irrational' to profess degrees of belief that do not obey these rules. But the rules are intuitively attractive and essential to our theory. . . . They are essential to our theory because only those set functions that obey them can be combined by Dempster's rule of combination.

Turning therefore for a defence of Dempster's rule we find (p. 57):

> Though this essay provides no conclusive *a priori* argument for Dempster's rule, we will see in the following chapters that the rule does seem to reflect the pooling of evidence . . .

It would appear, therefore, that the theory is to be taken as a whole and judged by its results: that is to say, whether it yields reasonable-seeming degrees of belief (whatever these might be). Nevertheless it seems possible to make some remarks on the conditions restricting degrees of belief, without first assessing the rule of combination, and this will be my first task.

I have already mentioned the approach to upper and lower probabilities by way of upper and lower betting rates. No doubt this involves a measure of idealisation. Nonetheless it appears to have some content and leads, through natural conditions of coherence, to well-defined classes of admissible upper and lower probability functions. It is the class of lower probability functions, $\mathscr{P}_*$

say, that corresponds to Shafer's class of belief functions though, as mentioned before, Shafer's class is in general a proper subset of $\mathscr{P}_*$ (always assuming a common domain of definition). $\mathscr{P}_*$ has the following structural property. Each non-empty subset has a greatest lower bound in $\mathscr{P}_*$ under the ordering

$$P_* \leqslant P'_* \quad \text{iff} \quad P_*(A) \leqslant P'(A) \quad \text{for every } A \subseteq \Theta.$$

The same is not true in general for Shafer's class. In fact the greatest lower bound of a non-empty subset $\mathscr{P}^0$ of $\mathscr{P}_*$ is given by

$$P^0_*(A) = \inf\{P_*(A) \colon P_* \in \mathscr{P}^0\} \quad \text{for every } A \subseteq \Theta.$$

P^0_* will always belong to $\mathscr{P}_*$ whereas in general it leads us outside Shafer's class.

This property of $\mathscr{P}_*$ means that if the lower probabilities of certain subsets of Θ are fixed, the class of all lower probability functions agreeing with these assignments, if non-empty, has a unique minimal element, namely its greatest lower bound, which clearly respects the initial assignments. Thus we can say that this function attributes to an arbitrary subset of Θ a degree of belief no greater than is already implied by the initial assignments: its existence in $\mathscr{P}_*$ means that this way of speaking makes sense. The fact that this is no longer true of Shafer's class affects the possibility of finding within that class a function which adequately expresses our degrees of belief (whatever they might mean) in certain cases. For suppose there is evidence relating to the unknown outcomes of two tosses of a coin giving rise, for each toss, to a belief function

$$Bel(\{H\}) = \tfrac{1}{2}$$
$$Bel(\{T\}) = 0.$$

The upper and lower probabilities of heads, on either toss, are therefore $\frac{1}{2}$ and 1, respectively. Now consider which belief function might be chosen to express the impact of the given evidence on the set of possible joint outcomes $\{HH, HT, TH, TT\}$. We must have

$$Bel(\{HH, HT\}) = Bel(\{HH, TH\}) = \tfrac{1}{2} \tag{5}$$
$$Bel(\{TH, TT\}) = Bel(\{HT, TT\}) = 0 \tag{6}$$

since the arguments in (5) are respectively the events 'heads on the first toss' and 'heads on the second toss', whilst the arguments in (6) refer correspondingly to tails. Furthermore, one can imagine situations in which it would seem reasonable to say that no more support accrues to the remaining sets of possibilities than is required by (5) and (6). That is to say, we should look for a minimum element in the set of belief functions satisfying these conditions. However it is not difficult to see that any non-negative solution of

$$Bel(\{HH\}) + Bel(\{HH, HT, TH\}) = 1$$
$$Bel(\{HH\}) \leqslant \tfrac{1}{2}$$

determines a minimal element. It follows that no ***minimum*** element exists, since the smaller the degree of belief accorded to $\{HH\}$, the larger must be the degree accruing to $\{HH, HT, TH\}$. In this case it is difficult to see how it would be possible to choose an appropriate belief function in a non-arbitrary way. The function it would be most natural to choose would assign to the two critical subsets $\{HH\}$ and $\{HH, HT, TH\}$ the least values they can separately assume, namely 0 and $\frac{1}{2}$. But this is not available within Shafer's class.

It might be replied that the evidence, in such a case, would have a bearing on the remaining possibilities which would permit a non-arbitrary choice. But it is difficult to see how this could be true in general. Suppose the evidence to consist of the outcome of a single toss of the coin. It is hard to see how this could provide evidence for or against any particular correlation. It is perhaps also worth mentioning that one of Shafer's criticisms of the (additive) Bayesian theory is its supposed inability to represent ignorance. 'The basic difficulty is that the theory cannot distinguish between lack of belief and disbelief. It does not allow one to withhold belief from a proposition without according that belief to the negation of the proposition' (p. 23). One could express this by saying that, in virtue of the requirement

$$Bel(A)+Bel(\bar{A}) = 1,$$

there is no minimum element in the relevant class of (additive) Bayesian belief functions. In view of the above, it seems that a similar objection can be raised against Shafer's theory.

So far I have attempted to criticize the theory without fixing on a precise interpretation of 'degree of belief', since none is given, and without mentioning the rule of combination. I now propose to argue that if degrees of belief are interpreted as lower betting rates, the rule of combination is incorrect. For this purpose I shall consider an application of the rule, already mentioned as an example of it, in which subsequent evidence establishes with certainty a proposition in the domain of the initial belief function. Let $\Theta = \{a, b, c\}$ and let *Bel* be the belief function over the subsets of Θ corresponding to the basic probability assignment of $\frac{1}{8}$ to every non-empty subset of Θ except for $\{b\}$ which receives $\frac{1}{4}$. Fixing $A = \{a\}$ and $B = \{a, b\}$, we have

$$Bel(A) = \tfrac{1}{8}$$

$$Bel(B) = \tfrac{1}{2}$$

$$Bel(A|B) = \tfrac{2}{7}.$$

Recall that $Bel(A|B)$ is the new degree of belief assigned to A if B is subsequently discovered to be true. More precisely, it is the value of A computed by the orthogonal sum $Bel \oplus Bel_B$. Now suppose that, for any sufficiently small stakes, $Bel(A)$ and $Bel(B)$ are the largest rates at which the subject is willing to bet on A and B, respectively, and that $Bel(A|B)$ is the largest rate at which he is willing to bet on A conditional on B, the bet being called off if B fails to occur. This means, for instance, that the subject is willing to accept a bet of 2 units at the rate $\frac{1}{2}$ on B together with a bet of 7 units at the rate $\frac{2}{7}$ on A conditional on B. But this is the same as an unconditional bet of 7 units at the rate $\frac{1}{7}$ on A. It necessarily has the same outcome. Consequently $\frac{1}{8}$ is *not* the largest rate at which the subject is prepared to bet on A, contrary to the original assumption. (The same follows only assuming bets at rates arbitrarily close to the critical values.) Notice that there is no requirement that the subject be 'reasonable', in the sense of the rates being coherent, beyond the need for him to recognise the identity of (possibly compound) bets.

There are a number of ways of meeting this objection. It could be argued that Bel_B is not the appropriate belief function for expressing the new evidence. Or it might be said that, for a betting interpretation, $Bel(A|B)$ as defined by

Shafer should be interpreted as the largest rate at which the subject would be prepared to bet on A if B were discovered to be true (whatever this means), whereas the interpretation given is in terms of the subject's prior readiness to accept conditional bets. Alternatively, one could just accept that degrees of belief in the intended sense are not interpretable as betting rates. But betting is only an elementary example of action in face of uncertainty. Unless belief and action are in some way related, and none other is suggested by Shafer, it is questionable whether quantified belief is a significant notion. Without arguing the matter further, I would be inclined to think that the fault lies with the rule of combination.

In order to do justice to the thoroughness of Shafer's theory it is necessary to say something more concerning the application of this rule. The orthogonal sum of two belief functions is defined, when it exists, by a mathematical operation. It is a separate question, however, whether that sum accurately expresses the combined effect of the two bodies of evidence on which the belief functions are separately based. For suppose Θ and Ω are two sets of possibilities, or 'frames of discernment', and that Ω is a refinement of Θ. That is to say, Ω is obtained from Θ by subdividing some of the possibilities discerned by Θ into further subcases. Now let Bel_1 and Bel_2 be belief functions defined over the subsets of the refinement Ω. Concerning the combined degrees of belief accruing to subsets of the coarser frame Θ, there is a choice. These may be computed either by means of the orthogonal sum of the restrictions of Bel_1 and Bel_2 to the coarser frame Θ or by means of the restriction to Θ of the orthogonal sum of Bel_1 and Bel_2 (calculated in Ω). In general a different answer results. In such a case, Shafer advocates the use of the finer frame. In general one should use the most refined frame possible. Briefly, Shafer only claims adequacy for the rule of combination if it is employed in a frame that 'discerns the relevant interaction' of the several bodies of evidence (pp. 57, 179). For such a frame, no further refinement will have any effect on the combined degrees of belief attributed to the possibilities which it discerns.

This restriction, however, is not sufficient for the correct application of the rule. This can be seen in an extreme case as follows. In the orthodox Bayesian theory it makes no difference if, through an oversight perhaps, one takes account of the same evidence twice. To show this one would argue, depending on the precise meaning of 'taking account of the same evidence twice', either that $P(A \mid B \cap B) = P(A|B)$ since $B \cap B = B$, or else that if P has been obtained from another distribution by conditionalising on B, then $P(B) = 1$ and hence $P(A|B) = P(A)$. Thus conditionalising on B has no further effect. The same is not true in Shafer's theory. For suppose that Bel is based on the body of evidence $\mathcal{E}$. (A curly $\mathcal{E}$ is used to indicate that the evidence may not be expressible as a proposition in the domain of Bel, or even as a proposition at all.) Now consider two copies of Bel and $\mathcal{E}$. The combined evidence is just $\mathcal{E}$ again. But the orthogonal sum $Bel \oplus Bel$ is in general quite different from Bel. It is presumably to avoid this difficulty that Shafer requires that the belief functions to be combined are based on 'entirely distinct bodies of evidence'. The full claim is that:

> The rule [of combination] does seem to reflect the pooling of evidence, provided only that the belief functions to be combined are actually based

> on entirely distinct bodies of evidence and that the frame of discernment discerns the relevant interaction of those bodies of evidence. (p. 57)

It is not clear, however, that this formulation is sufficient to distinguish unambiguously between permissible and impermissible applications of the rule. To begin with, the identity criteria for bodies of evidence are unclear if these cannot be expressed by propositions. Indeed, even if they can be, do two propositions which are not logically equivalent, but are nonetheless equivalent by virtue of natural laws, express 'entirely distinct bodies of evidence'? Or again, suppose that two bodies of evidence are distinct, taken as wholes, but nonetheless partly overlap. More explicitly, suppose that Bel'_1 and Bel'_2 are based on $\mathscr{E}'_1$ and $\mathscr{E}'_2$ but that these bodies of evidence partly overlap in $\mathscr{E}_0$, in the sense that they can be regarded as the conjunctions of $\mathscr{E}_0$ with $\mathscr{E}_1$ and $\mathscr{E}_2$ respectively. If Bel_1 and Bel_2 correspond to $\mathscr{E}_1$ and $\mathscr{E}_2$, the correct way of combining the evidence is presumably to take $Bel_0 \oplus Bel_1 \oplus Bel_2$ rather than $Bel'_1 \oplus Bel'_2$. But given arbitrary $\mathscr{E}'_1$ and $\mathscr{E}'_2$, how is one to extract the common part, given that bodies of evidence are not necessarily expressible as propositions?

This difficulty can be put another way. It seems that the rule of combination is appropriate, if at all, to cases of *independent* evidence. But, understandably, Shafer only rarely speaks in this way. The difficulty is that if 'independence' is to mean anything in this context, it must surely mean stochastic or probabilistic independence. But this notion is not available to Shafer, given his insistence on the hazy and non-propositional nature of evidence and his criticism of the Bayesian theory for requiring that the prior probability of the evidence be already established. This emerges rather clearly in the case of statistical inference.

Shafer considers the following situation. Suppose that the probabilities of outcomes of a certain experiment are dependent on a parameter θ whose value belongs to a finite set Θ. Assume furthermore that these conditional probabilities are given. (Shafer assumes a frequency interpretation for such probabilities and hence that they are additive.) The experiment might consist of drawing balls at random from an urn and noting the proportion of black balls in the sample. θ would then be the proportion of black balls in the urn. In general, what support does a given observation provide for θ belonging to any of the various subsets of Θ? Shafer proposes, as an answer, that the upper probability of any singleton $\{\theta\}$ should be set proportional to the probability of the observed outcome, conditional on the parameter assuming that value θ. Briefly, take upper probability proportional to likelihood. In addition, it is suggested that the belief function should be 'consonant' in the sense that the upper probability of any subset of Θ should be the maximum of the upper probabilities of its elements. This determines the constant of proportionality for singletons and hence, in an obvious way, the upper probabilities and thus the degrees of belief for all subsets of Θ.

The belief function determined in this way is understood to represent only the effect of the observed evidence. If there were prior evidence relating to the value of θ, the corresponding prior belief function should be combined with the belief function described above to yield the overall posterior belief function. Shafer points out that if the prior belief function is additive, as in the orthodox

Bayesian treatment, the posterior will coincide with the Bayesian solution. Otherwise he gives examples only of cases in which the prior belief function is vacuous. On the basis of a number of such examples he concludes that these 'should amply demonstrate the intuitive appeal of our method' (p. 245).

Consider a simple example. A coin is known to be biased, the probability of heads being either 0·9 or 0·3. The coin is tossed. Depending on whether we observe heads or tails, the resulting belief functions, according to the above proposal, are

$$\begin{aligned} &Bel_H(\theta = 0{\cdot}9) = \tfrac{2}{3}, \qquad Bel_H(\theta = 0{\cdot}3) = 0; \\ &Bel_T(\theta = 0{\cdot}9) = 0, \qquad Bel_T(\theta = 0{\cdot}3) = \tfrac{6}{7}. \end{aligned} \tag{7}$$

This means that the lower and upper probabilities of the bias being towards heads are ($\frac{2}{3}$, 1) given heads and (0. $\frac{1}{7}$) given tails.

There is nothing in these figures, or in any others Shafer presents, that is clearly counterintuitive. But they could be altered quite substantially without affecting this appearance. Clearly we need some more delicate method of appraisal to reach reliable conclusions. But here again we meet the difficulty that nothing precise is said about the intended meaning of 'degrees of belief', or what difference it would make settling on one set rather than another. One possibility, as before, would be to try to interpret $Bel_H(\theta = 0{\cdot}9)$, say, as the lower conditional probability that $\theta = 0{\cdot}9$ given heads and to interpret this, in turn, in terms of a subject's willingness to accept conditional bets. But then to be coherent in the sense described above, the conditional probability distribution now represented by (7) would have to be the lower envelope of a family of orthodox conditional probability distributions determined by a family of additive priors. But if the likelihoods are assumed to be fixed, it is clear that in this case, which is entirely typical, no such family exists. Hence the proposals are incoherent in this special sense. It might be objected that there is no intention of interpreting the expressions in (7) in this way. But if it is to be correct to claim (p. 251) that these proposals generalise the Bayesian treatment, as mentioned above, such expressions must, in that case at least, be interpreted in precisely this way.

There is another feature of the treatment of statistical evidence that deserves comment. Suppose that, in the case of the biased coin, several tosses are performed resulting in the outcomes $x_1, \ldots, x_n$. These separately determine belief functions $Bel_{x_1}, \ldots, Bel_{x_n}$ in the way indicated which can be combined by the rule of combination to represent the joint effect of the observations. Alternatively, given the joint distribution of $\mathbf{x} = (x_1, \ldots, x_n)$ for each value of θ, we can compute directly the resultant belief function $Bel_{\mathbf{x}}$, which also has a claim to represent the joint effect of the observations. Shafer only considers the case in which the observations are independent, in the sense that for each value of θ the joint distribution is just the product distribution, and observes that, in such a case, $Bel_{\mathbf{x}}$ is generally different from $Bel_{x_1} \oplus \ldots \oplus Bel_{x_n}$. Shafer regards these two belief functions as corresponding to interestingly different methods of evaluating the effect of the joint evidence. For the critic, however, it is difficult to avoid the conclusion that, if it is legitimate to apply the rule of combination in the case of 'physically independent' observations as Shafer does, there is a palpable contradiction between the rule of combination and the convention for dealing with statistical observations.

It is worth remarking that Bel_x depends explicitly on the correlations, or lack of them, between observations. The orthogonal sum is clearly independent of them. In this respect Bel_x seems preferable since common sense, as well as Bayesian ideas, would expect the effect of combined evidence to depend on the correlations between its parts. This is the main weakness of the rule of combination. It gives a unique answer to an incompletely specified question. Without demanding to know the dependencies between separate bodies of evidence, it pronounces categorically on their combined effect. This would be harmless, perhaps, if its use were only authorised in the case of some particular correlation —presumably that of independence. But it is very difficult to be convinced that this is the case.

There are two conditions for the use of the rule of combination (see above). First, the various bodies of evidence must be distinct. This is not problematic in the statistical case. Separate tosses presumably provide 'distinct' items of evidence (whatever the correlation). Secondly, the frame of discernment in which the orthogonal sum is calculated must 'discern the relevant interaction' of the various bodies of evidence. In the statistical case the frame in question is the set of possible parameter values. To say that it discerns the relevant interaction is to say, essentially, that one would obtain the same result no matter what further refinement of this frame were considered. In the coin tossing case, presumably, the relevant refinements can only be those which take into account the possible outcomes of the various tosses. It seems to me possible to exhibit cases of quite arbitrary correlations between the tosses for which, nonetheless, the parameter space 'discerns the relevant interaction'. If this is correct, the conditions circumscribing the use of the rule of combination are insensitive to the dependencies between the various bodies of evidence, and the criticism holds. Unfortunately, however, this is not a matter that can be settled with much certainty.

Lastly it is necessary to mention a rather different topic. It concerns not so much the structure of belief and its relation to evidence, but more the representation of the possibilities over which belief is distributed. I have already mentioned that, in order to apply the rule of combination in a given frame Θ, it is necessary to be assured that Θ is sufficiently refined. Thus it is necessary to conceive of a whole *family* of 'frames of discernment' and to imagine that the evidence in question determines a belief function for each. Only then is it possible, in principle, to test whether further refinement is necessary. For this purpose, Shafer introduces a mathematical structure $(\mathscr{F}, \mathscr{R})$ to represent such a family of frames with their associated refinings. The frames in $\mathscr{F}$ are just finite sets Θ such as we have been dealing with, and the refinings in $\mathscr{R}$ are maps between the frames, or rather their subsets. Their purpose is to keep track of which frames are refinements of which others, and which possibilities in one frame correspond to which sets of subpossibilities in another. It is demanded of such a family $(\mathscr{F}, \mathscr{R})$ that each two frames in $\mathscr{F}$ have a common refinement and that each frame individually can be indefinitely refined. In particular, for each frame Θ, for each possibility $\theta \in \Theta$ and for each positive integer n, there must exist a refinement of Θ in which θ splits into exactly n subpossibilities.

Shafer makes much of the fact that no 'ultimate refinement' appears in his scheme. For good reasons he criticises the idea of such a frame, so fine that it encompasses all possible distinctions:

> An ultimate refinement would have to be a complete account of the history of the universe, the very writing of which would require knowledge surpassing the collective experience of mankind (p. 120).

But oddly enough, he attributes the need for such a conception to the Bayesian theory.

> Advocates of the Bayesian theory usually assume the existence of an ultimate refinement, and though they sometimes excuse this assumption as a harmless mathematical simplification, it appears to be essential to their theory. It is certainly essential to the Bayesian claim that any new evidence can be assimilated by conditioning one's global Bayesian belief function (p. 119).

This seems to me much mistaken. Furthermore, Shafer's and the orthodox scheme have much more in common than he seems willing to admit.

The exact conditions characterising a 'family of compatible frames of discernment' are complicated. Indeed, reassurance that such families exist would be welcome. It is worth observing, therefore, that every atomless Boolean algebra yields such a family. It is enough, in effect, to identify the frames with the finite subalgebras and the refinements with the inclusion maps between them. In fact this is the general case. For every family $(\mathscr{F}, \mathscr{R})$ of compatible frames of discernment, there exists a set Σ and a field F of subsets of Σ such that the frames in $\mathscr{F}$ correspond one–one with the finite partitions of Σ into sets belonging to F. It is therefore misleading for Shafer to say (p. 119) that he rejects a scheme in which all frames are described as different partitions of the same set. Furthermore, to give a 'family of compatible belief functions' over $(\mathscr{F}, \mathscr{R})$ is just to give a real-valued function of the appropriate type over the field F. But this is just the usual scheme, except for the additional requirement that the field be non-atomic.

To what extent then is an ultimate refinement avoided? This depends on whether a 'family of compatible frames of discernment', considered as a whole, is understood to be capable of further refinement. If not, the elements of the associated set Σ will embody ultimate distinctions whether one likes it or not. The requirement that F be non-atomic merely decrees them to be unmentionable. If, on the other hand, the family as a whole can be further refined, the situation is the same as in the more usual scheme where one considers, relative to a given problem, a determinate set of possibilities over which probability is to be distributed. Relative to a different problem, a further subdivision of possibilities may be desirable and, from a mathematical point of view, can always be effected. This is a genuinely open-ended approach and one which accommodates the frequent Bayesian need to refine the possibilities previously considered in order to take account of fresh evidence. It does not follow, of course, that the probability function can always be extended over the whole of the refined field so long as strong conditions, such as countable additivity, are imposed. And indeed here we have the really significant point of divergence between restrictive and non-restrictive schemes. Provided Shafer retains a condition characterising belief functions that corresponds to finite additivity (subadditivity, or whatever), his scheme remains non-restrictive in this respect. With this in mind, it seems especially unfortunate that Shafer should have accused the Bayesian viewpoint of adopting a restrictive scheme involving ultimate refinements, when the writer who more than any other has stressed the need for a flexible and open-ended approach, and the consequent need to abandon countable additivity as a general requirement, is a distinguished proponent of that viewpoint, namely Bruno de Finetti.

In conclusion it seems to me that Shafer's 'mathematical theory of evidence' is based on a formal framework that has considerable appeal and naturalness from the mathematical point of view. But as a theory of probable reasoning, it is open to a number of objections. I have argued, first, that the formal restrictions on the structure of belief functions make it difficult, in certain cases, to find a belief function which might adequately express a subject's opinions. Secondly, the rule of combination, if correct in the case treated in the Bayesian theory, excludes the possibility of interpreting degrees of belief in terms of acceptable betting rates. In the absence of any other interpretation, it is hard to see how degrees of belief might be linked to dispositions to act in situations of uncertainty, and hence to know how to assess the practical merits of the proposals. Thirdly, I have argued that any rule for combining evidence should take into account the various possible dependencies between the bodies of evidence in question. Dempster's rule, which Shafer adopts, appears to be intended for the case of 'independent' evidence. But the usual notion of independence is not definable within the terms of the theory, given its view of the nature of evidence. Consequently, a surrogate proviso is adopted demanding 'distinctness' of the various bodies of evidence and the use of a frame which 'discerns their relevant interaction'. I have argued that it is very questionable whether this proviso has the desired effect.

Despite these reservations, we owe Shafer a debt of gratitude for developing a systematic and comprehensive alternative to the Bayesian theory. The problems with which it deals are of both practical and theoretical importance. The exposition is clear and straightforward with mathematical complications kept to a minimum, routine calculations being relegated to appendices. It makes rewarding reading and can be recommended to all critical students of probabilistic reasoning.

P. M. WILLIAMS
The University of Sussex

REFERENCES

DEMPSTER, A. P. [1967]: 'Upper and Lower Probabilities Induced by a Multivalued Mapping', *Annals of Mathematical Statistics*, 38, 325–39.

SMITH, C. A. B. [1961]: 'Consistency in Statistical Inference and Decision', *Journal of the Royal Statistical Society*, Series B, 23, pp. 1–25.

WILLIAMS, P. M. [1975]: 'Coherence, Strict Coherence and Zero Probabilities', *Contributed Papers*, Fifth International Congress of Logic, Methodology and Philosophy of Science, VI–29, 30.

WILLIAMS, P. M. [1976]: 'Indeterminate Probabilities', in M. Przełecki, K. Szaniawski and R. Wojcicki (*eds*): *Formal Methods in the Methodology of Empirical Sciences*, Ossolineum and D. Reidel Publishing Company, pp. 229–46.

Reviews

CHALMERS, A. F. [1976]: ***What is This Thing Called Science? An Assessment of the Nature and Status of Science and its Methods.*** Queensland: University of Queensland Press, and London: Prentice-Hall International. £3.70. Pp. xvii+157.

When teaching philosophy of science the author of the present volume became increasingly aware that there was no suitable single book, nor even a small number of books, that one could recommend to the beginner interested in modern views about the nature of science. In order to fill this gap Chalmers has set out to write a simple, clear and elementary introduction to modern philosophy of science.

The views presented are inductivism, falsificationism, Lakatos's and Kuhn's views, the author's own 'radical instrumentalism', and finally as 'two radical critiques' Althusser and Feyerabend. The book is written in a dialectical way. In the first chapter inductivism is presented, and in the next two chapters it is criticised. Then two chapters are devoted to falsificationism as at least a partial solution to the problems unsolved by inductivism, and in one chapter we are confronted with some problems of falsificationism. As an attempt to solve these problems the views of Lakatos and Kuhn are presented in two chapters. In one chapter Kuhn is criticised for being too subjectivistic. As a point of departure for Chalmers's own views there is a chapter criticising Popper's correspondence theory of truth. After this criticism of a realist position, we are ripe for Chalmers's own instrumentalism. In the last chapter Feyerabend's criticism of Lakatos is sketched, and Althusser's views are presented as a promising research programme in line with the author's own instrumentalism.

In the first chapter inductivism is presented as a widely held common-sense view saying that science is knowledge derived from the facts of experience. In this chapter Chalmers also accounts for the logical structure of explanations and predictions.

In the second chapter Chalmers shows that a principle of induction is necessary in order to derive universal hypotheses from singular facts of experience, and different formulations of such a principle are discussed. With reference to David Hume, Chalmers shows that a principle of induction cannot be empirically justified.

Another inductivist assumption is discussed in the third chapter, namely that science and inductive inferences start from a secure empirical basis. In order to show that observation statements are the results of active interpretations and not of passive perceptions different cases of *Gestalt* perceptions are discussed. Chalmers shows that in scientific research observation and experiment are not done by scientists standing like unprejudiced children before nature, but are guided by theory, and that observation statements are formulated in a theoretical language. As a result of the theory-dependence and our active interpretations, observation statements are fallible and do not constitute a secure empirical basis for inductive inferences.

The main advantage of falsificationism as presented in the fourth and fifth chapters is that it denies that science is based on induction and therefore overcomes the problems raised by a principle of induction. With ***modus tollens*** falsifications can be made with ordinary deductive logic. According to falsificationism 'this thing called science' consists of falsifiable hypotheses that have withstood severe tests. Science is not based on induction, but on 'conjectures and refutations'. Applying this method we cannot get absolute truth, but we can hope to make progress and to come closer to the truth.

The sixth chapter summarises Lakatos's criticism of falsificationism. Chalmers thinks that the fact that observation statements are fallible seriously undermines the fasificationist position. He correctly reports that Popper is aware of the fact that observation statements are fallible and their acceptance only is tentative and open to revision. But he thinks that Popper aims at conclusive falsification, and this of course is not possible with fallible observation statements. Now Popper had already written in his [1934] that no conclusive disproof of a theory can ever be produced (section 9). Therefore I think that Chalmers should have asked himself if falsificationism really presupposes infallible falsification, if falsificationism cannot function perfectly with tentative and fallible falsifications that in principle are always open to revision. I think that such ideas would fit very well with a criticalist view of science, and I wonder whether the idea that conclusive falsifications are necessary is not reminiscent of old justificationism, which in a weakened form says that at least falsifications must be absolutely justified and based on a rock bottom. The rest of the arguments in this chapter are also based on the assumption that falsifications must be conclusive. Chalmers tries to show that the possibility of using auxiliary and *ad hoc* hypotheses rules out conclusive falsifications, and that the history of science makes the ideal of infallible falsifications unrealistic.

The views of Lakatos and Kuhn are presented in the seventh and eighth chapters. Common to both is that they regard theories as 'structural wholes'. Both reject falsificationism, because they think, as Lakatos has put it, that every theory is surrounded by 'an ocean of anomalies'. Sailing in such treacherous waters, the best thing scientists can do is to orient their compasses according to the heuristic power of theories or research programmes. According to Kuhn the heuristic power of a paradigm is partly determined by its 'puzzle-solving' power. The future heuristic power is largely a matter of belief. When the confidence in a paradigm is shaken, scientists are thrown into a semi-religious crisis of belief until they are converted to a new and less problematic paradigm. Lakatos has tried to reduce the subjective element in Kuhn's thought, and to give more or less objective criteria for the progress and degeneration of research programmes. The main idea is that a research programme is progressive if it predicts new facts. The problem with this approach is how from the past performances of a research programme we can conclude anything about its future performances, if Kuhn's element of belief really can be eliminated. As Chalmers reports in the last chapter, Feyerabend's criticism of Lakatos aims at this weak point. How much time can elapse before a research programme has hopelessly degenerated and is incapable of leading to the discovery of new phenomena? Since Lakatos gives no time limits, Feyerabend and Chalmers think that Lakatos's methodology is 'anarchism in disguise'. Chalmers draws the somewhat paradoxical conclusion that '[a]ccording to the most sophisticated methodology, there is no methodology' (p. 145).

In order to overcome this unsatisfactory state of affairs, Chalmers presents his own 'radical instrumentalism'. First he criticises Kuhn's methodological ideas as being too subjectivistic in the ninth chapter. Popper's objectivism is rejected in the tenth chapter. The main criticism of Popper is that his correspondence theory of truth is untenable. Chalmers does not think that Popper has solved the semantical problem of what is meant by saying that a statement is true. He asks rhetorically: 'What do we wish to claim by the assertion that modern physics is true?' (p. 121). His answer is that we certainly do not mean correspondence with the facts. Therefore he thinks that Popper imports a common-sense idea of truth into science 'with no further argument or discussion' (p. 121). This argument depends on the assumption that we really want to claim that 'modern physics is true'. A fallibilist could answer that he would never dream of claiming such a thing, and that a scientific theory is accepted (temporarily) not because it is true, but because it is well corroborated.

'Radical instrumentalism' is the view offered by Chalmers in the eleventh chapter as a solution to the problems of modern philosophy of science. An advantage of this view is that it (like fallibilism) does not claim that theories are true descriptions of the world. In contradistinction to 'naive instrumentalism' 'radical instrumentalism' stresses the importance of 'ruthless testing' (p. 131). It gives an instrumentalist interpretation not only of theoretical language, but also of observational language. For the appraisal of competing theories criteria like simplicity and coherence are offered (p. 133).

In the last and most problematic chapter of the book Chalmers gives accounts of the ideas of the marxist Althusser and the anarchist Feyerabend. Having already described Feyerabend's criticism of Lakatos, Chalmers now goes on to present some of Feyerabend's extra-methodological views, for example that he is a hedonist who does not think 'that there is something special about science' (p. 144), but who nevertheless, in Chalmers's interpretation of his thoughts, thinks that anarchistic science would give 'delight . . . to its individual practitioners' (p. 147). As to methodology Feyerabend is a sceptic who thinks that 'everything goes'. This scepticism is underpinned by the already-mentioned criticism of Lakatos's methodology.

For Althusser the notion of 'scientific practice' is central. Certain social conditions are necessary for it. 'Its existence depends on its relationship with the other practices that make up the total social structure and will only continue as long as those other practices support it' (p. 137). But when these necessary conditions are fulfilled, then scientific practice is 'relatively autonomous'.

Two kinds of question can be raised concerning scientific practice, 'namely, questions concerning the social relations that are necessary for a particular autonomous science to exist, and questions concerning the functioning of that science as practice once it does exist' (p. 137). In the following, Chalmers only deals with questions of the first kind. Since most people would admit that there are some necessary social conditions for science, like financial means, appropriate institutions, a certain autonomy of science, and so on, these parts are rather unproblematic. But we do not get any answers to the second type of questions of what is characteristic for scientific practice. This is astonishing, since such answers would be most relevant in a book which tries to answer the question of what this thing called science is. Instead Chalmers repeats criticism of Popper and Lakatos, and assures us without showing how that with Althusser's ideas these problems certainly can be solved. So we are informed that Althusser's position, being 'materialistic', represents 'whole-hearted objectivity', while Popper and Lakatos are 'decidedly half-hearted' (p. 137). Popper's third world of theories, arguments, problems, *etc.* is rejected because it 'is somewhat akin to Plato's world of ideas' and therefore 'idealistic' (p. 139).

Now Chalmers knows that his 'briefest of sketches' of Althusser's ideas is rather vague. But he thinks that it is a promising research programme. Since the functioning of scientific practice is totally unclear, and since the heuristic power of the programme is in no way shown, the reader rather gets the impression that Althusser's 'research programme' is hopelessly degenerate.

Every chapter of the book ends with valuable recommendations for further reading which give commented references to original sources. There is a bibliography at the end of the book. There I missed Kuhn's main work, *The Structure of Scientific Revolutions*, which is extensively discussed in the book. I also missed indications of when Chalmers refers to editions different from the first one, as he obviously is doing when he for example writes that Popper's *Conjectures and Refutations* was published in 1969.

Disregarding the last chapter and the criticism of falsificationism, Chalmers has succeeded in writing a good introduction to the modern philosophy of science, of use to every student and teacher of the subject. With the exception of the unveiled mysteries of Althusser's scientific practice, the book is simply and clearly written.

GUNNAR ANDERSSON
University of Trier

REFERENCE

POPPER, K. R. [1934]: *Logik der Forschung*. Wien: Springer.

PORTER, ROY [1977]: *The Making of Geology: Earth Science in Britain 1660–1815*. London: Cambridge University Press. £8.95. Pp. 228.

It is not unusual for the creators of a new discipline within science to deny their intellectual ancestors, rather as most adolescents rebel at some stage against their parents. No doubt this is a necessary phase of growing up and should be viewed indulgently. What is extraordinary about the self-proclaimed founders of modern geology, from Playfair to Lyell, is that until very recently standard historiography has taken them at their word and accepted their contemptuous dismissal of earlier thinkers about the Earth as mere undisciplined speculators. Geology, according to Lyell, was not to be confounded with cosmogony.

Porter's aim in this excellent little volume is to counter what he regards as a fundamental misconception by charting the development of geological thought in Britain from the late seventeenth to the early nineteenth century when geology as currently understood is generally agreed to have become firmly established. By placing pre-nineteenth century scientists, scholars, natural philosophers, or whatever one wishes to call them, in their proper social, cultural and intellectual context he acquires a far more convincing perspective with which to view their achievements than has been obtained hitherto. Consider for instance the flood of theories about the Earth at the turn of the seventeenth and eighteenth centuries. Far from having exhibited what Playfair termed 'a species of mental derangement', many of the theorists in question were counted amongst the leading scientific intelligentsia of the day. We can readily acknowledge this in the case of Halley and Hooke, but it is instructive to learn that Burnet's work was much admired by Newton and Whiston's by Locke.

The creation of a scientific 'myth' about the Earth as a whole was an important advance on an earlier tradition that treated various mineral objects and geomorphological features in isolation, and the fact that less emphasis than later was placed on empirical verification does not detract from the significance of this phase in the growth of Earth Science. Theories remained teleological and anthropocentric, but under the influence of Newtonian mechanistic philosophy the emphasis gradually shifted from a planet in decay to one which was essentially stable and ordered, whether viewed as behaving passively or as a system involving dynamic equilibrium of forces. Early in the Hanoverian period the penchant for theorising declined, but rather than being a scientific dark age, as has been maintained, the eighteenth century saw the progressive broadening of the social base of geology, increasingly widespread dissemination of knowledge, growth of museum collections and founding of provincial scientific societies. This upsurge of provincial middle class culture was associated primarily with commercial and industrial enterprise, and the leading fieldworkers became as the years passed increasingly cognisant of the significance of rock strata, and hence learned to appreciate the great antiquity of the Earth.

This was the milieu which spawned William Smith. Those who founded the Geological Society of London came from a more elevated, principally Oxbridge-educated social stratum characterised by gentlemen of leisure and those engaged in the liberal professions. Both groups shared a deep distrust of all-embracing theory, reinforced by the counter revolutionary political climate in this country at the turn of the eighteenth and nineteenth centuries, and saw themselves as hard-headed, field-oriented empiricists espousing a Baconian philosophy.

One of the most illuminating sections of the book is the reevaluation of James Hutton. Seminal figure he may be, but far from being solely the undogmatic empiricist of geological folklore, he stood decidedly apart from the mainstream. Loftily dismissive of utilitarian science and as much a rationalist system builder as those of an earlier generation, Hutton embraced a deistic steady state philosophy of the Earth regardless of evidential support and can only properly be understood as a member of the Scottish Enlightenment along with his friends David Hume and Adam Smith.

Porter argues that geology, like any other science, is essentially an intellectual construct and hence a human artifact that was 'made' over the period in question; for this reason it developed differently in different countries. I find his writing style elegant and his basic thesis highly persuasive.

A. HALLAM
University of Birmingham

BODEN, MARGARET A. [1977]: *Artificial Intelligence and Natural Man.* Hassocks, Sussex: Harvester Press. Hardback: £13.50, Paperback: £4.95. Pp. xiii+537.

This is not a book aimed primarily at philosophers, and those looking for philosophical bones to pick will have to wade through a mass of clearly and entertainingly written descriptive details to find them. Dr Boden assumes a minimum of background knowledge in her readers, and displays admirable gifts as a teacher, especially in her choice of similes and worked examples. I particularly enjoyed her comparison of a computer programme with a knitting pattern!

'Artificial Intelligence' has been peaceably defined by Marvin Minsky as 'the science of making machines do things which would require intelligence if done by people'. Dr Boden uses the term in a more ambitious sense: 'The use of computer programs and programming techniques to cast light on the principles of intelligence in general and human thought in particular'. As such it falls well within the area of interest of the *B.J.P.S.* Indeed some of the earliest essays in the field now known as "A.I." were published in the first volumes of this Journal; and the philosophical issues debated then are evidently still with us. A relatively short section (pp. 418–44) is devoted explicitly to these, under the headings of: *Artificial Intelligence: A contradiction in terms?; Mind, Mechanism and Machines;* and *Can Machine Intelligence be achieved?* But Dr Boden's philosophical acumen is also evident at many other points.

The book falls into six main parts. The first clears the ground of some semantic boobytraps. The next four give detailed examples of programs to simulate personal characteristics (neurotic and otherwise), to handle ordinary language, to recognise and interact with objects in the visual world, and to learn to solve problems creatively. A final Part deals with psychological, philosophical and social implications of these developments. A dominant theme is that when properly understood, research in A.I. can help to counteract the dehumanising influence of natural science by forcing us to recognise the irreducible need for mental categories if intelligent behaviour is to be adequately simulated or understood. The subject/object distinction is fundamental. 'Any theoretical undertaking that hopes to express psychological realities by means of a (behaviorist or physiological) conceptual scheme that does not allow of this distinction is, therefore, doomed to failure' (p. 396). Reductionism of the "nothing-buttery" variety is self-defeating; but in fact the key terms, 'reduction' and 'mechanism', each have two different senses. Once these are distinguished, 'it is possible wholeheartedly to endorse the humanist stress on subjectivity in psychology without thereby jeopardising the mechanist's firm insistence that psychological phenomena depend ultimately on causal processes within the brain' (p. 397).

There may be nothing philosophically new in all this, but it is refreshing to watch the surefootedness with which Dr Boden guides her readers past the *canards* of debate in this area. While she has no time for the debunkers of human dignity, she is equally critical of those who try to save human dignity by finding "something that a machine can't do". Gödel's Theorem, for example, 'does *not* show that there must be some statements that could be known to be true by people, but not by programmed machines' (p. 434).

Perhaps she is least sure of her ground in dealing with Polanyi's suggestion that tacit knowledge is irreducible to explicitly programmatic terms: that we always "know more than we can say". First, I think it helps here to distinguish between the difficulty of putting ***our own*** tacit knowledge into words, and the difficulty of formally representing ***someone else's*** tacit knowledge. The first is likely (as Dr Boden admits) to be in principle unending, even though successive verbal approximations may yield continually growing insight. The second is another matter. All "knowing" by lower animals is by definition "tacit"; but this places no barrier in principle against ***our*** finding an effectively complete formal representation of what they know. By the same token, it is far from obvious that in principle my own tacit knowledge could not be completely formalised by an ideally-informed observer of my brain-workings, even though inevitably unformalisable by ***me***. Certainly (if one were not confined to devices run by explicit programmes) it is easy to envisage an automaton that would incorporate tacit knowledge which it could not formally express in its own language.

Secondly (and relatedly), although Dr Boden rightly criticises Hubert Dreyfus for excessive pessimism about the scope of discrete-symbol processors, she devotes only a marginal note to the enrichment of scope that could result from allowing discrete transitions in such devices to be determined (at least statistically) by ancillary physical interactions (effectively, the running of internal physical experiments) rather than by explicitly pre-determined processes of symbol-manipulation. Lack of suitable hardware has long hampered developments along these lines; but they would seem to offer the most natural analogue to some forms of information-processing now identified in the nervous system; and they would incidentally obviate one of the most persistent criticisms of discretely-programmed simulations—namely, that not all human behaviour is reducible to the following of explicit rules. To argue that even such stochastic processes could be in turn approximated by a large enough digital computing programme would be to miss the point; for any programme to compute the probabilities of transitions in a higher-level programme would be effectively in a ***meta-language*** relative to the rules of that programme.

To say all this is not really to criticise. What Dr Boden has set out to do she has done exceptionally well. She sums up cautiously (p. 444): 'The epistemological issues involved are too obscure to allow one . . . to insist that *all* aspects of human thought could in principle be simulated by computational means.'

One dangerous precedent is set in the book: the use of the pronoun 'she' to refer to such sexually neutral beings as 'the humanist' or 'the programmer'. Why dangerous? I tried it on verse 28 of Chapter 17 of the *Book of Proverbs*, and found the result too tendentious to reproduce here.

D. M. MACKAY
University of Keele

WESTMAN, R. S. (*ed.*) [1975]: *The Copernican Achievement*. Berkeley, Los Angeles and London: University of California Press. £9.45. Pp. xvi + 405.

The Copernican Revolution has for a long time been a conundrum for philosophers of science. Put most simply, the problem is this: at what date did it become scientifically justifiable, or "rational", to reject Ptolemaic astronomy *and* Aristotelian physics, in favour of the earth's motion about the sun? To earlier generations, the present truth of a theory was quite sufficient justification for adherence to it at any previous time. Indeed, the problem for reflective historians of science was to explain the persistence of any erroneous belief after the true doctrine had been revealed. This occasionally required some strong rewriting of history, with (for example) Copernicus and Galileo seen as truly legendary heroes.

Sadder now, and perhaps wiser too, we know that "instant rationality" in the assessment of theories is not to be sought. There then inevitably arises an awkward period of debate, in which at least one side espouses the "wrong" theory. Unless they can be shown to have used Method incorrectly, the philosopher is left with the uncomfortable conclusion that good methods may occasionally lead one into, or at least leave one in, bad science. Still worse: given the difficulties of articulating and defending a fledgling new theory (dramatically described by Imre Lakatos), it is possible that on occasion someone using wrong methods has advanced the right theory. A possible outcome is then that at some stage of the debate over a new theory, the honest scientists are wrong and the scientific swindlers right. What then of the heuristic and moral value of methodologies of science?

All this will be familiar to devotees of Paul Feyerabend. After the grievous loss of Imre Lakatos, we have no-one willing to try to rescue "scientific rationality" by means of a single schematism for debate that comprehends this enriched awareness. Even Lakatos' own later work, exemplified here by a joint paper with Elie Zahar, could only pose the problem without really advancing towards a solution. At this point history becomes quite useful to philosophers of science, though now a history done by scholars who are philosophically aware, or at least disenchanted of the mythology of science.

With these considerations in mind, I shall sample the contents of this volume, itself the record of an admirable Copernicus symposium held at U.C.L.A. in 1974. The central essay, by the editor himself, looks at the "heuristic" of two generations of students of Copernicus. He shows that they did not see their science as a two-person combat; and that the range of decisions on Copernicus' work was both broader and less sharply defined, than retrospective histories have believed. The first generation of "post-Copernican" astronomers, notably Rheticus and Reinhold, emphasised the computational advantages of the Copernican methods, and some tried (successfully) to translate the model back into geocentric terms. By the 1570s, a new generation was on the scene, possessing greater technical mastery. They could, and did, address the physical-cosmological questions; and the greatest of them produced the Tychonic compromise. Westman's account stops there; one can imagine similar reconstructions of the problem-situation for Kepler and Galileo and then beyond. The transformation of beliefs and commitments among professional astronomers, proceeding by quanta of decades, could then be shown to have a sort of moving rationality of problem and response.

There was more to the Copernican Revolution than professional astronomy; and to ignore these aspects that now seem "external" is to misread the evidence. A valuable essay on Osiander, by Bruce Wrightsman, puts his notorious preface in context. For Osiander was a leading scholar among the Reformers of religion; his "anti-realist" attitude towards astronomy was based on a firm belief on the inspired truth of the Bible. He has sometimes been considered as an astrologer; although this is incorrect, still he shared their commitment to a deeper meaning in the visible world, for which clues are provided by the mathematical sciences. Indeed, the wilful neglect of astrology by nearly all historians of astronomy has led to many errors of interpretation, among which that of Duhem is the most monumental. If we speak of philosophical problems of "physics of the heavens", we must recognise that all astronomers from Ptolemy to Kepler, with the exception of Copernicus, were committed to a science of celestial influences. Of course "errors" among scientists on such a scale pose really unpleasant problems for philosophers with an ideological commitment.

Another way of softening some of the philosophical paradoxes of the Copernican Revolution is to accept that astronomy in the Renaissance was not fully "matured". It was impossible for scientists to sift out the spurious historical data from the genuine; and they lacked effective control of inexactness ("error") in data, calculations and results. It is possible to accuse great astronomers of wilfully falsifying their work (Ptolemy is a recurrent target for such attacks). But if we appreciate that there was no foundation of standard, really irrefutable calculations against which to test the more difficult or speculative ones, we have more sympathy, better historical insight, and perhaps improved philosophical understanding as well. A study along these lines is made by Janice Henderson, on Reinhold and Copernicus; the problem of uncontrolled error is analysed succinctly by E. S. Abers and C. Kennel (both physicists at U.C.L.A., here making a most useful contribution for their colleagues).

Coming back to the problem of justifying the heliocentric system in its earliest days, my own speculation of fifteen years ago, on "the precession of the equinoxes" is given pride of place in a Popperian fashion: the first essay, by Curtis A. Wilson, is devoted to a refutation of my theory. Welcoming all criticism as good scientists should, I only remark that Wilson's alternative candidate, uniform circular motion, cannot function as a proof of anything cosmological at all. For a long time we have known of the bi-epicyclic Ptolemaic models of the fourteenth century Islamic astronomer, Ibn Al-Shatir, of which the models of the *Commentariolus* may not have been completely independent.

Neglecting many other significant essays, I finally turn to the paper by Lakatos and Zahar. With great wit and style, Lakatos outlines the problem, and the contributions of his beloved antagonists. But his treatment of the issues at stake in the later sixteenth century is sketchy in the extreme; and the novel historical point contributed by Zahar is really quite modest. And even an important content-increasing feature of a new theory is only one element among many in a judgment of its "progressiveness". I have argued that the early pro-Copernicans had only vague, aesthetic principles on their side, while the "conservatives" had good astronomical evidence (absence of detectable stellar parallax), physical reasoning (whose refutation required Huygens and Newton), and indeed common-sense as well. Perhaps now that Lakatos has gone, no-one will battle for a proof of the rationality of successful science. Stephen Toulmin's sane and sensible historical relativism will rule, and Paul Feyerabend will have to find a new campaign. It will not be so much fun to watch, but a more supple philosophy of science will have its own merits.

J. R. RAVETZ
University of Leeds

STROUD, B. [1977]: *Hume*. London: Routledge and Kegan Paul. £7.50. Pp. xii+280.

This is a splendid addition to the series entitled 'The Arguments of the Philosophers', edited by Ted Honderich. Stroud presents a unified interpretation and assessment of Hume's philosophy as a whole (but without mention of Hume on religion, economics, politics or history). It is philosophically exciting, clearly written and not unduly lengthy. I doubt, however, whether it will be intelligible to the virtual beginner in philosophy, as the dust jacket claims, for although Stroud briefly expounds Hume's main views before discussing them, the philosophical level of his discussion is high throughout. So, like any worthwhile philosophy, this demands considerable concentration from the reader, and I would hardly recommend it as a first introduction to Hume, except perhaps to the very able student who already has some knowledge of modern analytical philosophy. But the book will certainly be of interest to Hume scholars and to philosophers generally, for Stroud is insistent that Hume's views and arguments should not be dismissed for inadequate reasons, yet relentless in his pursuit of what he sees as the real deficiencies of Hume's positions.

The main overall feature of Stroud's interpretation is to take Hume as an empirical scientist of human nature, rather than a sceptic who reduced the assumptions of empiricism to absurdity, or a forerunner of logical positivism who brandished the analytic/synthetic fork and left conceptual analysis as the only legitimate task of philosophy. Building on the famous work of Kemp Smith, Stroud aims to present a more systematic and consistent naturalistic interpretation, taking quite literally the subtitle of the *Treatise*: 'An Attempt to Introduce the Experimental Method of Reasoning into Moral Subjects'. It does indeed seem very clear that Hume hoped to do for the human realm what he thought that natural scientists such as Newton had already done for the rest of nature. The distinction which Hume makes in his *Natural History of Religion*, between its foundation in reason and its origin in human nature, is one that is applied (in effect, if not in name) throughout his philosophy. Again and again, we find negative 'sceptical' arguments, showing how some fundamental concept or belief cannot be justified by either reason or experience, followed by positive 'naturalistic' theories about how it has its origins in some postulated universal psychological disposition in human nature. Thus the idea of necessary connection, present in every causal belief, is traced to a disposition to associate ideas which have been constantly conjoined. And, in general, Hume hoped that the principle of the association of ideas could do for the mental world what gravitation had done for the natural.

Stroud is sympathetic to the spirit of Hume's naturalistic programme (and refers to kindred themes in Quine and Wittgenstein) but he is highly critical of the details of Hume's execution of it. In most cases, he finds that the 'theory of ideas' which Hume uncritically took over from his empiricist predecessors severely handicaps his philosophical progress. Thus Hume assumes that having the idea of necessary connection consists in having a certain mental particular present to the mind, but the reality is much more complicated than this—we have a whole range of conditional and general beliefs, or dispositions to make judgments of certain kinds. And again, Hume presents the problems of personal identity as that of how to tie isolated perceptions into bundles, whereas the fact is that the very concept of a perception already involves that of the person who has the perception. On the question of the explanation of action, Stroud argues that Hume does *not* prove that a feeling or passion must be present for an action to occur, and suggests that the theory of ideas may have led Hume to confuse wanting something with having a feeling or passion (p. 166). On the quite different question of the origin of our rules of justice, Stroud diagnoses Hume's failure to derive them from individual self-interest in his over-simple assumptions about what our individual dispositions are (p. 217): we may have more of a natural concern for fairness than Hume allows for in his theory of what is natural.

At the end of the book, reviewing the prospects for a Humean naturalism unencumbered by a 17th–18th-century theory of ideas, Stroud still finds himself apparently caught in Hume's typical dilemma: scepticism when thinking philosophically, but inevitable common sense as soon as the mind turns from philosophical argument to the ordinary pursuits of life, be they dining, backgammon, conversation or whatever. To argue, with Hume, that our ideas of causal connections, the continued and distinct existence of material objects, personal identity, and of right and wrong, are only fictions which because of our own human nature we cannot help project into our conception of the world, even though nothing objectively there in the world justifies this, is surely to leave philosophy in a highly unsatisfactory state. At certain points, Stroud admits that Kant took the argument a stage beyond Hume (pp. 231–2, 236–7). But he still concludes that 'the curious student of human nature will inevitably find himself in this conflict' (the one just described). I suspect that Kant pointed the way out of this conflict, and I wonder whether someone can add to this series of 'Arguments of the Philosophers' and do as effective a job on Kant as Stroud has done on Hume. The task is a daunting one!

LESLIE STEVENSON
University of St Andrews

ENGELHARDT, H. T., Jr., and SPICKER, S. F. (*eds.*) [1975]: *Evaluation and Explanation in the Biomedical Sciences* and [1976]: *Philosophical Dimensions of the Neuro-Medical Sciences*, volumes 1 and 2 of *Philosophy of Medicine*, edited by H. T. Engelhardt and S. F. Spicker. Dordrecht: Reidel. Pp. 240 and 274, respectively.[1]

These two volumes are the proceedings of philosophy and medicine conferences held two and three years ago, respectively. The peculiarities of these volumes, their virtues, as well as their weaknesses, are traceable to this fact, together with the fact that it has been only recently that philosophy has rediscovered medicine. Since the re-emergence of the philosophy of (in, and for) medicine has, to a not inconsiderable extent, been midwifed by the editors of these two volumes, these volumes *do* give readers a good idea of the broad spectrum of work currently being done in it. However, there is nothing remotely resembling an orderly introduction to the subject to be found in either of these volumes. Furthermore, there are few, if any, papers in these volumes which can claim to be enduring definitive statements on the issues which they address. That being the case, one might wonder why one should attend to the volumes at all? The reason for doing so is the same as the reason one would wish to attend the conferences, *i.e.* to discover and participate in one of the most open and exciting areas in philosophy today.

---

[1] I am most grateful to Arthur Caplan for his criticisms of a draft of this review.

If there is no orderly introduction, this is no doubt because orderliness is not one of the strong points of this field. On the contrary, it inherits from the work of many different philosophical movements and perspectives, and addresses an equally astounding number of subject areas. Some, no doubt, will be dismayed at the unpredictable character of this vigorous intellectual mongrel, and will find it difficult to follow, let alone participate in, an intellectual enterprise where the methodology is in flux, where it is not immediately clear what literature will turn out to be most relevant to a given problem, and in short, where philosophical problems do not present themselves as well-defined puzzles. Others will find the challenge to radically rethink many philosophical issues and to examine or re-examine perspectives which are 'outside their areas' exhilarating, if somewhat awesome.

Therefore, my first point about these volumes is that they do provide a remarkably *good* introduction to the philosophy of medicine but not by providing an orderly introduction to this complex subject. Rather, these essays present a survey of issues, assumptions, and philosophical approaches which comprise the geneology of this discipline, together with attempted reformulations of these issues and attempts to synthesise those approaches which will be more adequate to medicine. Not all of the essays break new ground, indeed, some are manifestly essays by established figures working within their accustomed idioms, who have selected from among the many philosophical issues concerning medicine, those which happen to be manageable within these established modes. However, in such cases, the commentators, if not the main speakers themselves, have usually done a fine job of enlarging our understanding of the usefulness of these established approaches and assumptions in dealing with some problems or some aspects of some problems in the philosophy of medicine.

The paper which comes closest to providing an introductory sampling of the literature is Engelhardt's 'The Concepts of Health and Disease'. It is best understood as an introduction to the literature for, unlike many of Engelhardt's other papers, it contains little in the way of extended argument and, furthermore, ignores many distinctions among the positions it surveys. For example, positions on the ontological status of diseases are forced to fit into one of the following three categories: first, the view that diseases are like parasites lodged in the host's body (what traditionally was referred to as the ontological conception of disease[1]); second, the view that disease entities are universals which have independent reality (the thesis of Platonic realism with respect to disease entities); and third, the view that all that exist are particular diseased persons and it is merely that some are said to have "the same disease" (a nominalist position with regard to disease entities). No room is left even for Aristotelian realism or conceptualism.

[1] I have discussed this subject at some length in section two of my [1977b].

If the aim of the paper is understood as being to provide a sample of problems and positions in the philosophy of medicine and an introduction to the listing of medical ideas, the casual organisation of the essay is understandable, and the extensive bibliography is seen as an integral part of the paper. However, taken as a philosophical essay on concepts of health and disease, the paper appears disjointed and hasty, and its extensive bibliography looks like a mere embellishment. The commentator, Loretta Kopelman, has interpreted the paper as the usual sort of philosophical essay—indeed, it would be difficult for a commentator to do otherwise—and she tries to sort things out by distinguishing between the criteria we employ in deciding whether to consider some process a disease and those we employ in trying to explain the origin or development of such processes. She says that the former, but not the latter, requires normative judgment. I would argue that classification of states or processes ultimately must influence the explanation of these states or processes, since classification determines what sorts of processes and states we take to be *the same* sorts of processes and states, and, therefore, explainable in the same way.[1] Elsewhere I have argued that classification of diseases into types reflects certain interests and hence values, however even if Kopelman is mistaken in holding that the explanatory theories of medical science are entirely value-free, her distinction between the identification of a process as a disease and the explanation of it is useful, and those who find themselves confused by Engelhardt's paper will find Kopelman's paper helpful.

This pattern of complementation between speaker and commentator is frequently encountered in these two volumes, *i.e.* the virtues which may seem to be lacking in the main papers are generally present in the commentary papers and vice versa. Generally, the commentators evidence a commitment to successfully communicating with a large segment of the extremely diverse audience of humanists and health professionals. This has proved particularly important in those cases in which the main speaker was intent on treating a favourite subject in his or her accustomed idiom, with only a word or two relating their concerns to issues in medicine.

In *some* such cases, notably in Alasdair MacIntyre's 'How Virtues Become Vices', Hans Jonas's 'On the Power or Impotence of Subjectivity' and George Pitcher's 'Pain and Unpleasantness', the essays are polished at least in the treatment of their main themes. Therefore, readers whose tastes run more to the *established* breeds of philosophising rather than to their new and somewhat unpredictable issue, may find these the most successful individual papers.

The main thesis of MacIntyre's paper is that virtues are parts of social structure and thus that since human life exists in various social forms, there are, in addition to what he views as the central virtues necessary for all human life, other virtues which vary with the sorts of social relations peculiar to a society. So far, this is a very interesting thesis, but not one which is especially relevant to contemporary medicine. MacIntyre does try to make it relevant *via* his further claim that in contemporary society there are not enough shared beliefs about 'the true nature of man, and his true end, to yield any morality adequate to guide moral life'. He then argues that in particular, a moral code governing the practice of medicine will not be forthcoming in our fragmented society. MacIntyre does give us an explanation of why certain aspects of traditional medical ethics no longer have the moral force they once had. This explanation might have been used to show what was wrong with one simplistic contrast that was prevalent in some of the early literature in medical ethics. The contrast I have in mind is the one between a morally concerned dedication to the preservation of all life on the one hand, and an unfeeling devotion to expediency and convenience on the other. However, MacIntyre does not employ his explanation in this way. Since he sees the pluralism of modern American society as manifesting fragmentation rather than the emergence of a new set of values, he despairs of coming to any rationally guided decision on moral matters in general and moral problems in medicine in particular. The specific examples he gives of moral dilemmas in medicine are simplistic, and formulated in a rather sloppy manner. Samuel Gorovitz, in his reply, has no trouble giving much better examples, showing how philosophy may aid in the resolution of such dilemmas, and restoring the audience's confidence in the relevance of philosophy to specific moral problems, such as those which arise in medicine.

---

[1] See my [1978].

It would have been easy to argue against MacIntyre's assumption that there are no new values which arise from a commitment to pluralism and which replace those values which our society has discarded. (I submit that one such emerging value is embodied in the growing belief that those who must bear the consequences of a decision, are, to that extent, the ones who have the right to make that decision.) Although he does dispute MacIntyre's thesis that philosophy cannot resolve specific moral dilemmas, Gorovitz fails to address MacIntyre's assumption regarding the new values, and hence fails to examine the *reasons* for MacIntyre's despair about the ability of modern society to come to a decision on moral matters. I find this to be an omission; however, given the context of the conference I am not surprised, as some others may be, that Gorovitz neglects MacIntyre's central and carefully thought out thesis regarding virtues and social relations, and concentrates instead upon MacIntyre's application of his central thesis to the question of whether philosophy can help in the making of particular moral decisions.

Since, in their particular contributions, Jonas and Pitcher make less of a gesture in the direction of philosophy of medicine, I will not discuss their papers, except to remark that it is fortunate that, in his response to Jonas's paper, Stuart Spicker makes clear that Jonas concedes the materialist conception of matter *in order* to show the contradictions inherent in epiphenomenalism. Had Spicker not made this clear, those familiar with Jonas's thought might wonder why he concedes so much to the materialist position in this particular essay.

A new understanding of the person, and hence the elimination of old confusions about "minds" and "bodies" and their "relations" is certainly essential to the construction of an ontology adequate to medical practice and theory. However, the main effort in this direction is to be found in Marjorie Grene's 'Mind and Brain: the Embodied Person' and in Hubert Dreyfus's response to it, rather than in Jonas's essay. (In the same session with Grene's paper, there is a major paper by Karl H. Pirbram in which he wrestles with the problem of how mental representations get "projected" out into the "external" world. Dreyfus does an admirable job of identifying the principal philosophical confusions which underlie Pirbram's discussion.)

Grene's paper, together with Joseph Margolis's 'Persons and Psychosurgery' and perhaps David Bakan's 'Pain—the Existential Symptom' are the essays in the second volume which most genuinely represent the philosophy of medicine. In these we see the vigour and promise of this philosophical cross-breed. That the implications of the insights in these essays have not been either completely developed or honed to cohere with one another, is only to be expected, and if these papers fail to constitute enduring definitive statements on the issues, they do at least represent important contributions to such statements. Grene's central concern in her paper is to develop a notion of person to replace the concept of mind, and be more adequate for philosophical understanding in general, and issues in the philosophy of medicine, in particular. She means to firmly reject the attempt to understand the integrity or unity of a person in terms of a unitary consciousness, and thus in particular, to dismiss the suggestion that split-brain experiments reveal two minds and hence two people in a single body. Grene is unusually at home in dealing with many different philosophical traditions simultaneously, and as usual, she makes good use of many ordinary facts that get lost sight of in formulations current in contemporary philosophy. However, she does continue to make use of the terms 'mind' and 'body', and these terms may be misleading to those for whom these words have strong Cartesian connotations. After pointing out a number of points where Grene's account needs tightening up, Dreyfus proposes dropping the terms 'private', 'subject', 'consciousness', and 'mental' entirely. However, although we may need to use the term 'mental' more circumspectly, we are not likely to be able to frame the philosophical problems in neuro-medicine without it, and if we are worried about Cartesianism, we might better speak of 'organisms' rather than 'bodies'. If we make the latter revision of terminology, Grene's assertion that the mental depends upon a complex relation of *the organism* with culture, is less likely to be mistaken for epiphenomalism.

Margolis comes to a similar, though not identical position when he argues that the person is not a mere human animal (*i.e.* not a mere organism of a certain sort), but (at least) a culturally emergent entity. Margolis's paper is, however, not an account of the nature of persons; his full treatment of that subject appears in his [1978]. Here, he is concerned to give a careful account of the ethical and conceptual issues at stake concerning psychosurgery employing just a sketch of his forthcoming account of person. In this, he is largely successful, and, although in his commentary in Margolis's paper, Jerry Fodor is right in pointing out how much less we know about the functional organization of the brain than one would think from Margolis's discussion, it is Margolis, and not Fodor, who addresses the issues involved in psychosurgery. For the most part, Fodor merely contents himself with pointing out parallels between issues involving informed consent in the case of psychosurgery with those involved in the case of having ones dental cavities filled. While entertaining, Fodor's contribution to an understanding of issues in philosophy of medicine is confined to vividly setting forth the issue of a person's *right* to enter into a contract which entails undergoing any medical or surgical procedure he or she wishes.

It might be thought that Richard Zaner's paper, 'Context and Reflexivity: the Geneology of Self', would fit in with the discussions of Dreyfus, Grene, and Margolis, but I, for one, cannot see that it does. Although some phenomenological account of the self might well be to the point, it is not clear where Zaner is going with his. Even after the patient summary (and correction) offered by his commentator, Andre Schuwer, the whole of Zaner's paper remains unclear in itself, and thus of no clear relevance to the philosophy of medicine.

Against the background of the contributions by Grene, Margolis, and Dreyfus in the second volume, one can better understand what Stuart Spicker was aiming at in his paper, 'The Lived-Body As Catalytic Agent: Reactions At the Interface of Medicine and Philosophy', particularly his desire 'to establish an account of the human condition in terms of a strictly bodily account of existence' (p. 183). Without the benefit of the work of Grene and Margolis, Spicker's stated aim might look less like an escape from Cartesianism than the option for the materialist outgrowth of it, and Schuwer who comments on Spicker's as well as Zaner's paper, may be excused for mistaking the statement just quoted from Spicker for a concession to the 'demands of a symposium on the interface between philosophy and medicine' (p. 206). Something that strikes me as genuine error in Spicker's paper, and one which Schuwer does not address, is his reading of Kant as tracing the distinction between a form and its mirror image by reference to the bilateral minor symmetry of the human body. Although the passage which Spicker quotes from Kant is open to this interpretation, the rest of Kant's work, notably section thirteen of the *Prologemena*, shows Kant to have been struck by the fact that a form cannot be made congruent to its mirror image so long as one remains in space of the same number of dimensions, and to have taken this mathematical fact to show that space is not a property of things-in-themselves. It turns out in the general case that, in order to make an n-dimensional form congruent to its mirror image, one must move it in $n+1$-dimensional space. This is illustrated in the case of two dimensional forms, say, 2 dimensional right and left spirals, which can be made congruent only if one moves one out of the two-dimensional plane and flips it over. This mathematical result would be unchanged even if we humans were constructed like octopuses with a single eye in the top of our heads. However, there certainly are philosophers, like Poincaré, who are the heirs of Kant, and who *do* argue that the spatial structure of nature as we know it is a function of our anatomical makeup (among other things). Therefore, the sort of argument which Spicker wishes to attribute to Kant is certainly to be found elsewhere in philosophy.

Concerning the contributions of historians of medicine, Lester King, Chester Burns, and William Bynum, I shall merely remark that the papers by King and Burns provide historical overviews of concepts of health and disease which are useful as introductions to the subject, while Bynum makes an interesting scholarly contribution in his own area of expertise, nineteenth century history of medical ideas. Concerning the articles on hemispheric dominance and on measurement of pain responses by Arthur Benton and Bernard Tursky, I shall limit myself to warmly commending them to the attention of philosophers interested in the scientific findings on these subjects.

The three papers which I have left for last are those belonging to the section titled 'Philosophy of Science in Transition to a Philosophy of Medicine' in the first volume. Here we find contributions which, in different ways, are all distinctly efforts in the philosophy of medicine. Stephen Toulmin approaches the task by picking up the work of one of the great philosophers of medicine of the past, Claude Bernard, particularly Bernard's attempt to explicate what is distinctive about the functioning of living organisms without invoking any factor which is in principle outside the scope of scientific investigation (*e.g.* the 'vital principle'). To this end, Toulmin focuses upon the concepts of function and mechanism and makes a number of observations of the way these function in medical thinking. His central thesis is that there is no dichotomy between functions and mechanisms in natural processes. Indeed, he observes that accounts of local function are not readily distinguishable from accounts of the mechanisms involved. Toulmin's discussion of the factors which govern the choice of functional and mechanistic factors which we attend to when looking for causes and effects within physiological processes, is quite rough, surprisingly so since he regards this as 'the heart of the matter'. Reading the paper now, after the appearance of a number of recent papers on the concept of function, Toulmin's paper does not look like an important contribution. Moreover, it does not give as full a presentation of Bernard's views as one might wish. However, the effect of the paper at the time at which he presented it was to stimulate many to deal with the philosophical issues concerning causation and the concepts of function and mechanism as they appear in medical thought.

Patrick Heelan, in his comments, clarifies the principle muddle in Toulmin's paper, *viz.*: his conflation of an understanding of what constitutes the good of an organism with a commitment to the promotion of that good. In addition, Heelan suggests that the logic of relations between the languages of functional and mechanistic description is a complemented and non-distributed lattice, *i.e.* a quantum logic. However, it is not clear how this provocative suggestion squares with Toulmin's claim that accounts of local function are not readily distinguishable from accounts of the mechanisms involved, and Heelan does not discuss this point.

I have chosen to consider Marx Wartofsky's paper, 'Organs, Organisms, and Disease: Human Ontology and Medical Practice' last. I think that, of all the papers, one's appreciation of this one will vary most greatly with the understanding of the philosophy of medicine that one brings to these volumes. At first, it may seem that in claiming that the context for understanding disease is that of the broadest social and historical praxis, Wartofsky's account is programmatic to the point of being visionary. Certainly there is some justification for Heelan's fear that Wartofsky's approach leaves us resourceless and confused about the treatment of disease, if disease is so defined. However, particularly in the light of the papers by Grene and Margolis we can better understand Wartofsky's point about human ontology, *i.e.* we see the extent to which we are cultural beings. Therefore, it is clearer why dealing with the whole person is *already* to encounter that social and historical context, and thus, that we are not asked to redirect our attention away from people in considering this context.

The hope that it will be possible to so thoroughly rethink the relevant issues and reform the relevant practice that we may realise the kind of integrated approach to the promotion of health which Wartofsky envisions surely is not a modest one. However, since many in the medical community are acutely aware that the present fragmented approach frequently has the effect of injuring in one way, while aiding in another, the movement to undertake such a project is well under way. Therefore, this rethinking and reform may be practically as well as theoretically possible and something to which philosophers may contribute. Thus the hope which underlies Wartofsky's programme is not groundless.

The papers in these volumes reflect the special achievement of philosophy of medicine, that, unlike so many other philosophies of 'X', it has taken its interdisciplinary charge seriously. The result is an exchange in which philosophers and professionals from other specialties work together and learn significantly from one another. It further provides a forum in which philosopher's from many different schools, fields, and traditions exchange problems, perspectives and methods in a way which is rarely sponsored elsewhere in philosophy today, and which is likely to benefit philosophy, generally.[1]

CAROLINE WHITBECK
The Institute for the Medical Humanities
University of Texas Medical Branch at Galveston

REFERENCES

MARGOLIS, J. [1978]: ***Persons and Minds.*** Dordrecht: Reidel.

WHITBECK, C. [1977*a*]: 'The Relevance of Philosophy of Medicine for the Philosophy of Science' in F. Suppe and P. Asquith (*eds.*): *PSA 1976*, vol. 2. Ann Arbor: Edwards Brothers. Pp. 123–35.

WHITBECK, C. [1977*b*]: 'Causation in Medicine: The Disease Entity Model', ***Philosophy of Science*, 44**, 4.

WHITBECK, C. [1978]: 'The Relation between Theory and Practice in Medicine and Psychotherapy' in Horowitz, Lyner and Stein (*eds.*): *Science and Psychotherapy.* Haven Press.

[1] I have addressed the specific issue of the relevance of philosophy of medicine for the philosophy of science in a paper with that title [1977*a*].

407–412 Printed in Great Britain

Brit. J. Phil. of

blications

Towards a Rational Philosophical Anthropology. The Hague: Martinus
. 57.50. Pp. 404.

: *Morals and Politics—The Ethics of Revolution*. Henley: Routledge and
aul. £2.95. Pp. 170.

77]: *Reason and Action*. Dordrecht: D. Reidel Publishing Co. $24.00. Pp. 202.

H. [1978]: *Chase, Chance and Creativity*. New York: Columbia University
. $16.20. Pp. 237.

BADEA, F. [1977]: *Reaction Mechanisms in Organic Chemistry*. Tunbridge Wells: Abacus Press. £18.50. Pp. 701.

BARTHES, R. [1977]: *Sade, Fourier, Loyola*. London: Jonathan Cape. £5.95. Pp. 184.

BECKER, L. C. [1977]: *Property Rights: Philosophic Foundations*. Henley: Routledge and Kegan Paul. £4.95. Pp. 135.

BELL, J. [1978]: *Boolean-valued Models and Independence Proofs in Set Theory*. London: Oxford University Press. £5.95. Pp. 126.

BELL, J. and MACHOVER, M. [1977]: *A Course in Mathematical Logic*. Amsterdam: North-Holland Publishing Co. £13.50. Pp. 600.

BENTON, T. [1977]: *Philosophical Foundations of the Three Sociologies*. Henley: Routledge and Kegan Paul. £6.50. Pp. 225.

BERGER, R. [1978]: *Psyclosis: Circularity of Experience*. Reading: W. H. Freeman and Co. Ltd. £7.90. Pp. 221.

BERMAN, M. [1977]: *Social Change and Scientific Organisation: The Royal Institution 1799–1844*. London: Heinemann Educational Books. £11.00. Pp. 224.

BERNIER, R. and PIRLOT, P. [1977]: *Organe et Fonction*. Paris: Maloine s.a. Editeur. Fr. F. 84. Pp. 164.

BIEMEL, W. [1977]: *Martin Heidegger: An Illustrated Study*. Henley: Routledge and Kegan Paul. £5.75. Pp. 206.

BLAKEMORE, C. [1977]: *Mechanics of the Mind*. London: CUP. Hardback: £10.50. Paperback: £3.95. Pp. 208.

BODEN, M. [1977]: *Artificial Intelligence*. Hassocks, Sussex: The Harvester Press Ltd. Hardback: £13.50. Paperback: £4.95. Pp. 537.

BORKOWSKI, L. [1977]: *Formale Logik*. München: C. H. Beck Verlag. DM. 38. Pp. 578.

BRIDGE, J. [1977]: *Beginning Model Theory: The Completeness Theorem and some Consequences*. London: Oxford University Press. £4.50. Pp. 143.

BROWN, A. C. (*ed.*) [1977]: *A History of Scientific Endeavour in South Africa*. Rondebosch: Royal Society of South Africa. £12.95. Pp. 515.

BROWN, H. I. [1977]: *Perception, Theory and Commitment: The New Philosophy of Science*. Illinois: Precedent Publishing Co. $15.95. Pp. 200.

BROWN, R. H. [1977]: *A Poetic for Sociology*. London: CUP. £10. Pp. 302.

BRUSH, S. [1977]: *The Kind of Motion We Call Heat, Vols. I and II*. Amsterdam: North-Holland Publishing Co. $75.00 two volume set. Vol. I Pp. 299, Vol. II Pp. 769.

BUNGE, M. [1977]: *Treatise on Basic Philosophy, Vol. 3*. Dordrecht: D. Reidel Publishing Co. Hardback: $39.50. Paperback: $22.00. Pp. 343.

BUTTS, R. E. and PITT, J. C. (*eds.*) [1977]: *New Perspectives on Galileo*. Dordrecht: D. Reidel Publishing Co. Hardback: $29.50. Paperback: $12.50. Pp. 257.

BUTTS, R. E. and HINTIKKA, J. (*eds.*) [1977]: *Historical and Philosophical Dimensions of Logic, Methodology and Philosophy of Science*. Dordrecht: D. Reidel Publishing Co. $39.00. Pp. 332.

BUTTS, R. E. and HINTIKKA, J. (*eds.*) [1977]: *Basic Problems in Methodology and Linguistics*. Dordrecht: D. Reidel Publishing Co. $39.00. Pp. 318.

BUTTS, R. E. and HINTIKKA, J. (*eds.*) [1977]: *Logic, Foundations of Mathematics, and Computability Theory*. Dordrecht: D. Reidel Publishing Co. $39.50. Pp. 406.

BUTTS, R. E. and HINTIKKA, J. (*eds.*) [1977]: *Foundational Problems in the Special Sciences—Part 2*. Dordrecht: D. Reidel Publishing Co. $39.00. Pp. 422.

CANTORE, E. [1977]: *Atomic Order: An Introduction to the Philosophy of Microphysics*. Massachusetts: MIT. Paperback: $6.95. Pp. 334.

CANTORE, E. [1977]: *Scientific Man—Humanistic Significance of* ISH Publications. $20.00. Pp. 487.

CAPPELLETTI, A. [1977]: *La Teoria Aristotelica de la Vision*. Caracas: Soc olana de Ciencias Humanas. Pp. 96.

CARROLL, L. [1977]: *Symbolic Logic*. New York: Clarkson N. Potter Inc. $14.95. Pp.

CAUSEY, R. L. [1977]: *Unity of Science*. Dordrecht: D. Reidel Publications Co. D.fl. Pp. 208.

CERF, W. and HARRIS, H. S. (*eds.*) [1977]: *Hegel: Faith and Knowledge*. New York: State University of New York Press. $15.00. Pp. 206.

CHAKRAVORTY, K. R. [1977]: *Science Based on Symmetry—1*. Calcutta: Firma KLM Private Ltd. 150 Rupees. Pp. 502.

CHALMERS, A. F. [1977]: *What is This Thing Called Science?* Hemel Hempstead: Prentice Hall International. £3.70. Pp. 153.

CHATTOPADHYAYA, D. P. [1977]: *Individuals and Worlds: Essays in Anthropological Rationalism*. London: Oxford Books. £5.75. Pp. 227.

COBB, E. [1977]: *The Ecology of Imagination in Childhood*. Henley: Routledge and Kegan Paul. £5.75. Pp. 139.

COHEN, L. J. [1977]: *The Probable and the Provable*. London: Oxford University Press. £9.50. Pp. 363.

COHEN, R. S. and ELKANA, Y. (*eds.*) [1977]: *Hermann von Helmholtz: Epistemological Writings*. Dordrecht: D. Reidel Publishing Co. Hardback: $24.00. Paperback: $12.00. Pp. 215.

COLEMAN, W. [1978]: *Biology in the Nineteenth Century*. London: CUP. Hardback: £8.50. Paperback: £2.75. Pp. 187.

COLLIER, A. [1977]: *R. D. Laing: The Philosophy and Politics of Psychotherapy*. Hassocks, Sussex: The Harvester Press Ltd. Hardback: £8.95. Paperback: £2.95. Pp. 214.

DAVIES, P. C. W. [1977]: *Space and Time in the Modern Universe*. London: CUP. Hardback: £6.50. Paperback: £2.95. Pp. 232.

DAVISON, I. [1978]: *Values, Ends and Society*. Herts.: Prentice Hall International. £10.75. Pp. 257.

DUMMETT, M. [1977]: *Elements of Intuitionism*. Oxford: Clarendon Press. £9.00. Pp. 467.

DUNN, J. M. and EPSTEIN, G. [1977]: *Episteme 2—Modern Uses of Multiple-Valued Logic*. Dordrecht: D. Reidel Publishing Co. Pp. 338.

EARMAN, J. S., GLYMOUR, C. N., STACHEL, J. J. (*eds.*) [1977]: *Foundations of Space-Time Theories*. Minnesota: University of Minnesota Press. $22.50. Pp. 459.

ENGLEHARDT, H. T. and SPICKER, S. F. (*eds.*) [1977]: *Mental Health: Philosophical Perspectives*. Dordrecht: D. Reidel Publishing Co. $19.95. Pp. 298.

ERIKSEN, T. [1976]: *Bios Theoretikos: Notes on Aristotle's Ethica Nicomachea X, 6–8*. Oslo: Universitesforlaget. $14.00. Pp. 272.

FARLEY, J. [1977]: *The Spontaneous Generation Controversy from Descartes to Oparin*. London: Johns Hopkins University Press. £10.25. Pp. 224.

FREY, G. (*ed.*) [1977]: *Bela Juhos: Selected Papers on Epistemology and Physics*. Dordrecht: D. Reidel Publishing Co. Hardback: $39.50. Paperback: $19.50. Pp. 350.

FRIED, Y. and AGASSI, J. [1976]: *Paranoia: A Study in Diagnosis*. Dordrecht: D. Reidel Publishing Co. Hardback: $19.50. Paperback: $12.00. Pp. 230.

GEBER, B. A. (*ed.*) [1977]: *Piaget and Knowing—Studies in Genetic Epistemology*. Henley: Routledge and Kegan Paul. £5.75. Pp. 258.

GELWICK, R. [1978]: *The Way of Discovery: an Introduction to the Thought of Michael Polanyi*. Oxford: Oxford University Press. £9.80. Pp. 181.

GIEDYMIN, J. (*ed.*) [1977]: *Kazimierz Ajdukiewicz: The Scientific World-Perspective and Other Essays 1931–1963*. Dordrecht: D. Reidel Publishing Co. $49.50. Pp. 369.

COCHET, P. [1978]: *Quine en Perspective*. Paris: Flammarion. Pp. 229.

GOLDMAN, A. [1977]: *A Theory of Human Action*. New Jersey: Princeton University Press. Hardback: £10.10. Paperback: £3.15. Pp. 230.

GOLDMAN, L. [1977]: *Lukacs and Heidegger: Towards a New Philosophy*. Henley: Routledge and Kegan Paul. £3.50. Pp. 106.

GOODMAN, N. [1977]: *The Structure of Appearance*. Dordrecht: D. Reidel Publishing Co. $32.00. Pp. 276.

GOODY, J. [1977]: *The Domestication of the Savage Mind*. London: CUP. Hardback: £7.50. Paperback: £2.50. Pp. 179.

Gould, S. J. [1977]: *Ontology and Phylogeny*. London: Harvard University Press. £12.95. Pp. 501.

Grandy, R. [1977]: *Advanced Logic for Applications*. Dordrecht: D. Reidel Publishing Co. Pp. 167.

Granit, R. [1977]: *Purposive Brain*. Mass.: MIT Press. $12.50. Pp. 244.

Grant, E. [1978]: *Physical Science in the Middle Ages*. London: CUP. Hardback: £6.95. Paperback: £1.95. Pp. 128.

Grattan-Guinness, I. [1977]: *Dear Russell—Dear Jourdain*. London: Duckworth. £14.00. Pp. 234.

Graves, J. C. [1977]: *The Conceptual Foundations of Contemporary Relativity Theory*. Cambridge, Mass.: MIT Press. Paperback: $5.95. Pp. 358.

Gregory, G. [1977]: *Scientific Materialism in Nineteenth Century Germany*. Dordrecht: D. Reidel Publishing Company. Hardback: $28.00. Paperback: $13.50. Pp. 271.

Gruner, R. [1977]: *Theory and Power: On the Character of Modern Science*. Amsterdam: B. R. Gruner Publishing Co. Dfl. 30. Pp. 238.

Harnett, W. E. (*ed.*) [1977]: *Systems: Approaches, Theories, Applications*. Dordrecht: D. Reidel Publishing Co. $32.00. Pp. 198.

Hagar, W. A. [1977]: *Cognitive Awareness and the LPM*. New York: Philosophical Library. $12.00. Pp. 244.

Hall, A. R. and Tilling, L. (*eds.*) [1978]: *Correspondence of Isaac Newton: Vol. VII*. London: CUP. £30.00. Pp. 522.

Harris, E. E. [1977]: *Atheism and Theism*. New Orleans: Tulane University. $6.50. Pp. 157.

Harris, H. S. and Cerf, W. (*eds.*) [1977]: *Hegel: Difference Between Fichte's and Schelling's System of Philosophy*. New York: State University of New York Press. $15.00. Pp. 213.

Harnad, S., Steklis, H. and Lancaster, J. (*eds.*) [1976]: *Origins and Evolution of Language and Speech*. New York: The New York Academy of Sciences. $59.00. Pp. 914.

Harsanyi, J. C. [1976]: *Essays on Ethics, Social Behaviour and Scientific Explanation*. Dordrecht: D. Reidel Publishing Co. $36.50. Pp. 254.

Hayek, F. A. [1978]: *New Studies in Philosophy, Politics, Economics and the History of Ideas*. Henley: Routledge and Kegan Paul. £7.25. Pp. 314.

Hessing, S. (*ed.*) [1978]: *Speculum Spinozanum 1677–1977*. Henley: Routledge and Kegan Paul. £17.50. Pp. 590.

Hintikka, J. [1977]: *Knowledge and the Known: Historical Perspectives in Epistemology*. Dordrecht: D. Reidel Publishing Co. Pp. 243.

Hintikka, J. [1977]: *The Semantics of Questions and the Questions of Semantics*. Amsterdam: Elsevier, North-Holland. $17.00. Pp. 200.

Höffe, O. Forschner, M. and Schöpf, A. and Vossenkuhl, W. [1977]: *Lexikon der Ethnik*. München: C. H. Becksche. DM 14.80. Pp. 287.

Holdcroft, D. (*ed.*) [1978]: *Papers in Logic and Language*. Coventry: University of Warwick. £1.50. Pp. 177

Hollis, M. [1977]: *Models of Man-Philosophical Thoughts on Social Action*. London: CUP. Hardback: £7.00. Paperback: £2.50. Pp. 198.

Holton. G. [1978]: *The Scientific Imagination*. London: CUP. Hardback: £15.00. Paperback: £5.95. Pp. 282.

Hookway and Pettt (*eds.*) [1978]: *Action and Interpretation*. London: CUP. £5.95. Pp. 178.

Hörmann, H. [1977]: *Psychologie der Sprache*. Berlin: Springer-Verlag. $16.60. Pp. 223.

Hoyle, F. [1977]: *The Faces of the Universe*. London: Heinemann Educational Books. £4.80. Pp. 207.

Hoyle, F. [1977]: *On Stonehenge*. London: Heinemann Educational Books Ltd. £4.50. Pp. 126.

Hutchison, T. W. [1977]: *Knowledge and Ignorance in Economics*. Oxford: Basil Blackwell. £7.00. Pp. 186.

Jungermann, H. and de Zeeuw, G. (*eds.*) [1977]: *Decision Making and Change in Human Affairs*. Dordrecht: D. Reidel Publishing Co. $26.00. Pp. 512.

Kasher, A. (*ed.*) [1976]: *Language in Focus: Foundations, Methods and Systems*. Dordrecht: D. Reidel Publishing Co. Hardback: $64.00. Paperback: $29.00. Pp. 679.

KAUFFMAN, G. B. (*ed.*) [1976]: *Classics in Coordination Chemistry—Part II Selected Papers (1798–1899)*. New York: Dover Publications. $4.50. Pp. 174.

KEKES, J. [1976]: *A Justification of Rationality*. New York: State University of New York Press. $20.00. Pp. 275.

KITTS, D. B. [1977]: *The Structure of Geology*. Dallas: SMU Press. $8.95. Pp. 180.

KNIGHT, D. [1977]: *The Nature of Science: The History of Science in Western Culture Since 1600*. London: Andre Deutsch. £4.95. Pp. 215.

KRAJEWSKI, W. [1977]: *Correspondence Principle and Growth of Science*. Dordrecht: D. Reidel Publishing Co. $19.50. Pp. 136.

LACAN, J. [1977]: *The Four Fundamental Concepts of Psycho-Analysis*. London: Hogarth Press. £8.50. Pp. 290.

LAUDAN, L. [1977]: *Progress and Its Problems—Towards a Theory of Scientific Growth*. Henley: Routledge and Kegan Paul. £5.95. Pp. 257.

LAZEROWITZ, M. [1977]: *The Language of Philosophy: Freud and Wittgenstein*. Dordrecht: D. Reidel Publishing Co. Hardback: $24.00. Paperback: $14.00. Pp. 206.

LEMAINE, G. MACLEOD, R., MULKAY, M. and WEINGART, P. (*eds.*) [1976]: *Perspectives on the Emergence of Scientific Disciplines*. The Hague: Mouton. Pp. 282.

LEMMON, E. J. [1977]: *An Introduction to Modal Logic*. Oxford: Basil Blackwell. £4.95 Pp. 94.

LUBRANO, L. [1977]: *Soviet Sociology of Science*. Columbus: American Association for the Advancement of Slavic Studies. Paperback: $4.95. Pp. 102.

McGUINNESS, B. (*ed.*) [1976]: *Friedrich Waismann: Philosophical Papers*. Dordrecht: D. Reidel Publishing Co. Hardback: $26.50. Paperback: $16.50. Pp. 224.

MACKENZIE, B. D. [1977]: *Behaviourism and the Limits of Scientific Method*. Henley: Routledge and Kegan Paul. £4.95. Pp. 193.

MACHLUP, F. (*ed.*) [1977]: *Essays on Hayek*. Henley: Routledge and Kegan Paul. £4.25. Pp. 182.

MANDELBROT, B. B. [1977]: *Fractals: Form, Chance and Dimension*. California: W. H. Freeman and Co. $14.95. Pp. 352.

MANFREDI, U. (*ed.*) [1976]: *Crisi Dell'Occidente e Fondazione Della Cultura*. Palermo: Fondazione Andrea Biondo Centro Internazionale D'Iniziativa Culturale e Studi Filosofica Michele Federico Sciacca. Pp. 279.

MANIER, E. [1977]: *The Young Darwin and His Cultural Circle*. Dordrecht: D. Reidel Publishing Co. Hardback: $24.50. Paperback: $11.95. Pp. 237.

MARCUCCI, S. [1977]: *Kant e le Scienze—scritti scientifici*. Padova: Liviana Editrice. L 6,600. Pp. 298.

MARGOLIS, J. [1977]: *Persons and Minds*. Dordrecht: D. Reidel Publishing Co. Hardback: $26.00. Paperback: $11.95. Pp. 301.

MEDAWAR, P. B. and J. S. [1977]: *The Life Science*. London: Wildwood House Ltd. £4.95. Pp. 196.

MENDELSOHN, E. WEINGART, P. and WHITLEY, R. (*eds.*) [1977]: *The Social Production of Scientific Knowledge*. Dordrecht: D. Reidel Publishing Co. Hardback: Dfl. 65. Paperback: Dfl. 13.50. Pp. 300.

MOHR, H. [1977]: *Lectures on Structure and Significance of Science*. Berlin: Springer-Verlag. DM 38.20. Pp. 227.

MORRIS, D. [1977]: *Manwatching: A Field Guide to Human Behaviour*. London: Jonathan Cape. £7.95. Pp. 320.

MORSCHER, E., CZERMAK, J. and WEINGARTNER, P. (*eds.*) [1977]: *Problems in Logic and Ontology*. Graz: Akademische Druk-U Verlagsantstalt. Pp. 310.

MUNZ, P. [1977]: *The Shapes of Time*. Connecticut: Wesleyan University Press. $20.00. Pp. 382.

NARLIKAR, J. [1977]: *The Structure of the Universe*. London: Oxford University Press. Hardback: £4.25. Paperback: £1.95. Pp. 264.

NEDELJKOVIC, M. [1976]: *David Hume: Approche Phenomenologique de l'Action et Theorie Linguistique du Temps*. Paris: Presses Universitaires de France. Pp. 216.

NEU, J. [1978]: *Emotion, Thought and Therapy*. Henley: Routledge and Kegan Paul. £6.95. Pp. 194.

NEYMAN, J. (*ed.*) [1977]: *The Heritage of Copernicus*. Cambridge, Mass.: MIT Press Paperback: $9.95. Pp. 542.

NICHOLAS, J. (*ed.*) [1977]: *Images, Perception and Knowledge*. Dordrecht: D. Reidel Publishing Co $38.00. Pp. 305.

Pappas, G. and Swain, M. (*eds.*) [1978]: *Essays on Knowledge and Justification.* New York: Cornell University Press. Paperback: $6.95. Pp. 380.

Pattee, H. (*ed.*) [1973]: *Hierarchy Theory.* New York: George Braziller Inc. Hardback: $6.95. Paperback: $2.95. Pp. 156.

Petrov, U. [1977]: *Methodological Research.* Pp. 224.

Phillips, D. L. [1977]: *Wittgenstein and Scientific Knowledge: A Sociological Perspective.* London: Macmillan Press. £10.00. Pp. 248.

Pitcher, G. [1977]: *Berkeley.* Henley: Routledge and Kegan Paul. £7.50. Pp. 271.

Playfair, G. L. and Hill, S. [1978]: *Cycles of Heaven.* London: Souvenir Press. £4.95. Pp. 368.

Popper, K. R. and Eccles, J. C. [1977]: *The Self and Its Brain—An Argument for Interactionism.* Berlin: Springer-Verlag. DM 39. Pp. 597.

Porter, R. [1977]: *The Making of Geology 1660–1815.* London: CUP. £8.95. Pp. 288.

Przelecki, M. and Wojcicki, R. (*eds.*) [1977]: *Twenty-five Years of Logical Methodology in Poland.* Dordrecht: D. Reidel Publishing Co. $49.00. Pp. 803.

Przelecki, M. and Wojcicki, R. (*eds.*) [1977]: *Formal Methods in the Methodology of Empirical Science.* Dordrecht: D. Reidel Publishing Co. $39.50. Pp. 460.

Pujia, R. [1977]: *Bertrand Russel e L'Eredita Idealista Inglese.* Messina: La Libra. Pp. 213.

Putnam, H. [1978]: *Meaning and the Moral Sciences.* Henley: Routledge and Kegan Paul. £4.95. Pp. 145.

Ragg, N. [1977]: *People not Cases.* Henley: Routledge and Kegan Paul. £4.25. Pp. 152.

Ramsey, F. P. [1978]: *Foundations: Essays in Philosophy, Logic, Mathematics and Economics.* Henley: Routledge and Kegan Paul. £9.50. Pp. 287.

Reichenbach, H. [1977]: *Philosophie der Raum-Zeit-Lehre.* Berlin: Vieweg. DM 52. Pp. 442.

Reichenbach, H. [1977]: *Laws, Modalities and Counterfactuals.* London: University of California Press. £8.05. Pp. 140.

Reichenbach, H. [1977]: *Der Aufstieg der Wissenschaftslichen Philosophie.* Wiesbaden: Vieweg. DM 56. Pp. 490.

Rescher, N. [1976]: *Plausible Reasoning.* Assen: Royal Vangorcum Ltd. Dfl. 29.50. Pp. 124.

Rescher, N. [1977]: *Methodological Pragmatism.* Oxford: Basil Blackwell. £10.50. Pp. 315.

Rescher, N. [1977]: *Dialectics: A Controversy-Oriented Approach to the Theory of Knowledge.* New York: New York Press. $15.00. Pp. 128.

Rosenkrantz, R. D. [1977]: *Inference, Method and Decision.* Dordrecht: D. Reidel Publishing Co. Hardback: $26.00. Paperback: $11.95. Pp. 206.

Runciman, W. G. (*ed.*) [1978]: *Weber—Selection in Translation.* London: CUP. Hardback: £19.50. Paperback: £3.50. Pp. 398.

Schirn, M. (*ed.*) [1976]: *Studies on Frege I—Logic and Philosophy of Mathematics.* Stuttgart: Friederich Frommann Verlag. Pp. 317.

Schirn, M. (*ed.*) [1976]: *Studies on Frege II—Logic and Philosophy of Language.* Stuttgart: Friederich Frommann Verlag. Pp. 303.

Schirn, M. (*ed.*) [1976]: *Studies on Frege III—Logic and Semantics.* Stuttgart: Friederich Frommann Verlag. Pp. 201.

Schlesinger, G. [1977]: *Religion and Scientific Method.* Dordrecht: D. Reidel Publishing Co. $22.00. Pp. 190.

Schumacher, E. F. [1977]: *A Guide for the Perplexed.* London: Jonathan Cape, £3.95. Pp. 165.

Swartz, S. (*ed.*) [1977]: *Naming, Necessity, and Natural Kinds.* Cornell University Press. Hardback: £11.25. Paperback: £3.95. Pp. 277.

Sengupta, A. [1977]: *Language, Structure and Meaning.* Santiniketan: Pisva-Bharati. 46 Rupees. Pp. 113.

Shapin, B. and Goly, L. (*eds.*) [1977]: *The Philosophy of Parapsychology.* New York: Parapsychology Foundations Inc. $13.50. Pp. 295.

Simon, H. [1977]: *Models of Discovery.* Dordrecht: D. Reidel Publishing Co. Hardback: $39.50. Paperback: $19.95. Pp. 456.

Spurling, L. [1977]: *Phenomenology and the Social World.* Henley: Routledge and Kegan Paul. £6.95. Pp. 208.

Stegmüller, W. [1977]: *Collected Papers on Epistemology, Philosophy of Science and History of Philosophy Vols. I and II.* Dordrecht: D. Reidel Publishing Co. Both volumes $56.00. Pp. xxvii+525.

STROUD, B. [1977]: *Hume*. Henley: Routledge and Kegan Paul. £7.50. Pp. 280.

STUCHLIK, M. (*ed.*) [1977]: *The Queen's University Papers in Social Anthropology: Vol. 2 Goals and Behaviour*. Belfast: Mayne Boyd and Son Ltd. £2.00. Pp. 166.

SUPPE, F. (*ed.*) [1977]: *The Structure of Scientific Theories*. University of Illinois Press. $10.00. Pp. 818.

SUPPE, F. and ASQUITH, P. D. (*eds.*) [1977]: *PSA 1976 Vol. 2*. Michigan: Philosophy of Science Association. $9.75. Pp. 618.

TARG, R. and PUTHOFF, H. [1977]: *Mind Reach—Scientists Look at Psychic Ability*. London: Jonathan Cape. £4.50. Pp. 230.

THOMSON, J. Jarvis [1978]: *Acts and Other Events*. Cornell University Press. £10.50. Pp. 274.

THORPE, W. H. [1978]: *Purpose in a World of Chance: A Biologist's View*. Oxford: Oxford University Press. £3.95. Pp. 124.

TRAGESSER, R. S. [1977]: *Phenomenology and Logic*. Cornell University Press. £7.30. Pp. 135.

TROELSTRA, A. [1977]: *Choice Sequences—A Chapter of Intuitionistic Mathematics*. London: Oxford University Press. £4.95. Pp. 170.

TUOMELA, R. (*ed.*) [1977]: *Dispositions*. Dordrecht: D. Reidel Publishing Co. $38.00. Pp. 448.

TUOMELA, R. [1977]: *Human Action and Its Explanation*. Dordrecht: D. Reidel Publishing Co. $39.50. Pp. 417.

TURCHIN, V. F. [1977]: *The Phenomenon of Science: A Cybernetic Approach to Human Evolution*. New York: Columbia University Press. $21.90. Pp. 348.

TURNER, D. [1978]: *Commitment to Care*. Connecticut: The Devin-Adair Co. $12.50. Pp. 415.

TYMIENIECKA, A. T. (*ed.*) [1977]: *Analecta Husserliana Vol. VI—The Self and the Other*. Dordrecht: D. Reidel Publishing Co. $24.00. Pp. 186.

ULLMANN-MARGALIT, E. [1978]: *The Emergence of Norms*. London: Oxford University Press. £8.00. Pp. 206.

VICKERS, J. [1976]: *Belief and Probability*. Dordrecht: D. Reidel Publishing Co. $18.50. Pp. 190.

WASHCHKIES, L. J. [1977]: *Von Eudoxos zu Aritsoteles*. Amsterdam: B. R. Grüner Publishing Co. Dfl. 90. Pp. 453.

WESTFALL, R. S. [1978]: *The Construction of Modern Science: Mechanisms and Mechanics*. London: CUP. Hardback: £7.95. Paperback: £2.50. Pp. 171.

WESTMAN, R. S. and MCGUIRE, J. E. [1977]: *Hermeticism and the Scientific Revolution*. Los Angeles: William Andrews Clark Memorial Library. $5.00. Pp. 150.

WILDEN, A. [1972]: *System and Structure—Essays in Communication and Exchange*. London: Social Science Paperbacks: £5.25. Pp. 540.

WILKS, K. V. [1978]: *Physicalism*. Henley: Routledge and Kegan Paul. £4.75. Pp. 142.

WILLIAMS, L. Pearce and STEFFENS, H. J. [1977]: *History of Science in Western Civilisation Vol. II*. Washington: University Press of America. $9.75. Pp. 345.

WINNER, L. [1977]: *Autonomous Technology: Technics-out-of Control as a Theme in Political Thought*. Cambridge, Mass.: MIT Press. $17.50. Pp. 386.

YOSHIDA, R. [1977]: *Reduction in the Physical Sciences*. Halifax: Dalhousie University Press. Pp. 90.

ZIMAN, J. [1977]: *The Force of Knowledge: The Scientific Dimension of Society*. London: CUP. £4.75. Pp. 374.

ANNOUNCEMENT CONCERNING PITTSBURGH SERIES

The Center for Philosophy of Science at the University of Pittsburgh announces that its Series in Philosophy of Science, formerly edited by Robert Colodny, is to be expanded in both scope and frequency under the imprint of the University of California Press. The new Pittsburgh Series in Philosophy and History of Science, under the general editorship of Adolf Grünbaum and Larry Laudan (assisted by an Editorial Committee), will henceforth publish single-author monographs in all areas of the philosophy of science as well as conceptually-oriented studies of the history of science. But the Series will also continue the more familiar multi-author anthologies. The Editors welcome inquiries from authors, which should, in general, be addressed to Professor Larry Laudan, Center for Philosophy of Science, University of Pittsburgh, Pittsburgh, PA 15260.

SIXTH INTERNATIONAL JOINT CONFERENCE ON ARTIFICIAL INTELLIGENCE

IJCAI–79, the Sixth International Joint Conference on Artificial Intelligence will be held in Tokyo, August 20–24 1979.

Deadline for submission of papers to Programme Committee: 1 February 1979.

Contact: Dr Pat Hayes, Programme Committee Co-Chairman (Europe), Department of Computer Science, University of Essex, Colchester CO4 3SQ, Essex. (0206) 44144, ext. 2371 or 2325; or Professor Raj Reddy, General Chairman, Department of Computer Science, Carnegie–Mellon University, Pittsburgh, Pennsylvania 15213.

Sponsor: International Joint Conference on Artificial Intelligence.

THE PHILOSOPHER'S INDEX

An International Quarterly Index
To Philosophical Periodicals

The Philosopher's Index is an up-to-date quarterly index of articles from more than two hundred ninety international philosophy and interdisciplinary journals. Professional philosophers carefully index all articles, which are listed alphabetically by subject and author. Abstracts of the articles are published in each issue and are generally written by the authors of the articles. In addition, the ***Index*** contains a Book Review Index and a section entitled Translations in Progress.

The Quarterly, Paperback, $40 (Individuals: $20)
Annual Cumulative Edition, Hardbound, $50 (Individuals: $25)
Retrospective Index, Hardbound, $195 (Individuals: $130)

U.S. and Canada:

THE PHILOSOPHER'S INDEX
Bowling Green State University
Bowling Green, Ohio 43403 USA

Outside the U.S. and Canada:

D. Reidel Publishing Company/Dordrecht, Holland

INQUIRY *Editor: Alastair Hannay*
Founding and Consulting Editor: Arne Naess

Vol. 21, No. 2, Summer 1978

Nuclear Energy and Obligations to the Future	R. and V. Routley
Emotions and Anthropology: The Logic of Emotional World Views	Robert C. Solomon
Incommensurable Paradigms and Psycho-Metaphysical Explanation	J. Douglas Rabb
On the Action of Teams	David Londey
Frege's Realism	Gregory Currie
The Ins and Outs of Teleology: A Critical Examination of Woodfield (Andrew Woodfield: *Teleology*)	Larry Wright
The Liberal University (Alan Montefiore [Ed.]: *Neutrality and Impartiality: The University and Political Commitment*)	Peter Mew

Vol. 21, No. 3, Autumn 1978

Defining Civil Disobedience	Brian Smart
Social Realism and Social Idealism: Two Competing Orientations on the Relation between Theory, Praxis, and Objectivity	Tronn Overend
A Critique of Popper's Conception of the Relationship between Logic, Psychology, and a Critical Epistemology	John Krige
Rising Up from Downunder. Comments on Feyerabend's 'Marxist Fairytales from Australia'	Walter Suchting
The Ontological Status of Mental Images	Robert Audi
Three Essays on Violence (Ted Honderich: *Three Essays on Political Violence*)	H. J. N. Horsburgh

Subscription: Institutions – N.kr. 90.00, £6.25, $18.00, postage included.
Individuals – N.kr. 60.00, £5.00, $12.00.

Published quarterly by UNIVERSITETSFORLAGET, P.O. Box 7508,
Skillebekk, Oslo 2, Norway – P.O. Box 142, Boston, Mass. 02113, USA

GRAZER
PHILOSOPHISCHE STUDIEN

BAND 5 1978 VOLUME 5

Die Philosophie Franz Brentanos

Brentano and His Place in Austrian Philosophy	Edgar MORSCHER
The World in Which Brentano Believed He Lived	George KATKOV
Über Brentanos Reismus und die extensionale Logik	Stephan KÖRNER
Quantification and Brentano's Logic	Burnham TERRELL
Der scholastische Kontext des Intentionalen bei Brentano	Klaus HEDWIG
Brentano and Husserl on Intentional Objects and Perception	Dagfinn FØLLESDAL
On the Significance of the Correspondence Between Franz Brentano and Edmund Husserl	Herbert SPIEGELBERG
François Brentano et la Pensée Philosophique en Pologne	Izydora DAMBSKA
Intentionality of Thought Versus Intentionality of Desire	Peter GEACH
Will and Emotion	Elizabeth ANSCOMBE
Brentanos antinaturalistische Grundlegung der Ethik	Heiner RUTTE
Zur Erkenntnistheorie von Franz Brentano	Guido KÜNG
Brentano's Criticism of the Correspondence Theory of Truth	Paul WEINGARTNER
Brentano's Conception of Substance and Accident	Roderick M. CHISHOLM
Brentanos Sprachkritik oder daß „man unterscheiden muß, was es (hier) zu unterscheiden gibt"	Rudolf HALLER
Brentanos Analyse des Zeitbegriffes	Johann GÖTSCHL
Brentano's Relation to Aristotle	Rolf GEORGE

Herausgeber *Editor*

Prof. Dr. Rudolf HALLER, Philosophisches Institut, Universität Graz, Heinrichstraße 26, A-8010 Graz, Österreich/Austria.

the review of

metaphysics

a philosophical quarterly

SEPTEMBER 1978 | VOL. XXXII, No. 1 | ISSUE No. 125 | $5.00

articles

J. N. FINDLAY	Time and Eternity
MARJORIE GRENE	The Paradoxes of Historicity
THOMAS LANGAN	Searching in History for the Sense of It All
RALPH McINERNY	Can God be Named by Us?
NICHOLAS CAPALDI	Hume as Social Scientist
MANLEY THOMPSON	Peirce's Verificational Realism

books received

DEREK CROSS and Staff	Summaries and Comments

philosophical abstracts *doctoral dissertations* *visiting philosophers from abroad*
north american philosophers abroad *professors emeriti* *announcements*

Individual Subscriptions $13.00 Institutional Subscription $18.00 Student Subscriptions $9.00

Jude P. Dougherty, Editor
The Catholic University of America, Washington, D. C. 20064
Twenty-Year Cumulative Index, 1947–1967 is available for $3.00 a copy.

AMERICAN PHILOSOPHICAL QUARTERLY

PRIZE ESSAY FOR 1980

Topic:

THE ONTOLOGICAL STATUS
OF THE
LAWS OF NATURE

Deadline:

JANUARY 15, 1980

Prize:

$250 and publication in the OCTOBER 1980 issue of the APQ

Conditions:

Only current subscribers to the APQ are eligible to make submissions. These must be written in English. They should be presented in the standard APQ typescript format and must not exceed 7,000 words in length. Entries will be judged by a panel of readers appointed by the editor of the APQ. The right of declining to make any award is reserved if no submissions of suitable quality are received.

Volume LXXXVIII **MIND—JANUARY 1979** Number 349

Edited by D. W. Hamlyn

Articles:

Form, Species and Predication in Metaphysics Z, H and Θ — M. J. Loux
Sense-impressions: A New Model — M. Charlesworth
Characterising Self-deception — A. Palmer
Historical Counterexamples and Sufficient Cause — R. Martin
What Is an Intentional State? — J. R. Searle

Discussions:

On Quine's Contretemps of Translation — M. Yudkin
Clear and Distinct Perception and Metaphysical Certainty — P. Markie
Causal Sufficiency — G. Vision
'X's Reason for Φ-ing Was p' — G. F. Schueler
The Universalizability of Moral Judgments Revisited — M. E. Levin
General Terms and Common Resemblances — P. L. McGee and W. Slauson

Books Received

Notices

Published for the MIND ASSOCIATION by BASIL BLACKWELL
Orders to: Journals Department, Basil Blackwell & Mott Ltd.
108 Cowley Road, Oxford OX4 1JF, England

Annual subscriptions: Institutions—£8.00 or $16.00
Individuals—£6.00 or $12.00
Single issues—£225 or $4.50

AUSTRALASIAN JOURNAL OF PHILOSOPHY

EDITOR: BRIAN ELLIS

QUARTERLY PUBLICATION

The journal will be published *quarterly* from Vol. 57, 1979, onwards, with issues appearing in March, June, September and December.

POLICY

The journal will publish original articles of high quality in any area of philosophy which is judged to be of sufficient interest to its readers. It will also publish discussion notes, critical notices and reviews of important recent publications in philosophy.

NEW SUBSCRIPTION RATES

With the change to quarterly publication, it is anticipated that the journal will publish about 30 per cent more material annually. From 1979 onwards the following subscription rates will apply:

Individuals:	A$12.00	US$14.00	£Stg 7.00
Institutions:	A$18.00	US$20.00	£Stg 10.00
Students:	A$ 6.00	US$ 7.00	£Stg 3.50

COMMUNICATIONS

Manuscripts, subscriptions and inquiries should be addressed to the Editor, Department of Philosophy, La Trobe University, Bundoora, Melbourne, Victoria, Australia, 3083.

EDITORIAL BOARD

R. B. Braithwaite
G. Buchdahl
L. J. Cohen
H. Dingle
Sir John Eccles
M. B. Hesse
Sir Harold Jeffreys
W. C. Kneale
S. Körner
J. L. Mackie
D. J. O'Connor
Sir Karl Popper
H. R. Post
J. W. N. Watkins
G. J. Whitrow
J. H. Woodger

EDITORIAL ADVISORY PANEL

The Editors are helped by members of the Editorial Board, members of the Committee of the British Society for the Philosophy of Science and by an Advisory Panel which includes:

Peter Clark
Jon Dorling
Arthur Fine
Michael Gardner
Donald Gillies
Ian Hacking
Colin Howson
Clive Kilmister
Noretta Koertge
Alan Musgrave
Vernon Pratt
Ian Roxburgh
Peter Urbach
Elie Zahar

THE BRITISH SOCIETY FOR THE PHILOSOPHY OF SCIENCE

Committee 1978-9

President: Mr L. J. Cohen
President Elect: Dr G. Buchdahl
Hon. Secretary: Mr A. Young
Hon. Treasurer: Mr D. W. Miller
Members: Mr D. Hirschmann, Dr M. L. G. Redhead, Dr Y. Wilks, Dr P. M. Williams

The British Journal for the Philosophy of Science is the official organ of the Society, whose purpose is described by the following statement from its Constitution and Rules :

' The purpose of the Society is to study the logic, the method and the philosophy of science as well as those of the various special sciences, including the social sciences.'

The Society holds meetings, at which papers are read and discussed, once a month during the academic year. These are held, by kind permission, in University College, London. The Committee may recognise Regional Groups of Members formed to hold meetings in other parts of the country. Any Regional Group so recognised may nominate a representative Member to the Committee.

Ordinary membership is open to all interested persons on payment of the annual subscription. Membership of the Society is international and is by no means confined to people resident in the United Kingdom The annual subscription is £2.50 for U.K. residents and £1.25 for overseas residents ($2.50 if resident in U.S.A. or Canada). Those who wish to join should write to the Hon. Secretary, Mr Alistair Young, School of Social Sciences, University of Sussex, Falmer, Brighton BN1 9QH.

Brit. J. Phil. Sci. Vol. 29 No. 4 December 1978

The British Journal for the Philosophy of Science

Contents

Articles

311–323 JON DORLING Did Einstein need General Relativity to solve the Problem of Absolute Space? Or had the Problem already been solved by Special Relativity?

325–346 KLAUS HEILIG Carnap and de Finetti on Bets and the Probability of Singular Events: The Dutch Book Argument Reconsidered

Discussions

347–349 JOHN T. BLACKMORE Is Planck's 'Principle' True?

349–353 W. G. V. ROSSER The Clock Hypothesis and the Lorentz Transformations

353–355 GEOFFREY SAMPSON A Dilemma Defended

355–361 MICHAEL REDHEAD Ad Hocness and the Appraisal of Theories

Review Articles

363–375 JOHN MACKIE Failures in Criticism: Popper and his Commentators (review of P. A. Schilpp (*ed.*): *The Philosophy of Karl Popper*)

375–387 PETER WILLIAMS On a New Theory of Epistemic Probability (review of Glenn Shafer: *A Mathematical Theory of Evidence*)

Reviews

389–406 GUNNAR ANDERSSON on A. F. Chalmers: *What is This Thing Called Science?*; A. HALLAM on Roy Porter: *The Making of Geology*; D. M. MACKAY on Margaret Boden: *Artificial Intelligence and Natural Man*; J. R. RAVETZ on R. S. Westman (*ed.*): *The Copernican Achievement*; LESLIE STEVENSON on B. Stroud: *Hume*; CAROLINE WHITBECK on H. T. Engelhardt and S. F. Spicker (*eds.*): *Evaluation and Explanation in the Biomedical Sciences* and *Philosophical Dimensions of the Neuro-Medical Sciences*.

Recent Publications

Announcements

Printed in Great Britain at the University Press, Aberdeen